

Psychological Bulletin

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Psychological Bulletin

A HISTORY OF INTROSPECTION

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A proper but cumbersome title for this article would be "The History of the Availability of Consciousness to Observation in Scientific Psychology." If conscious experience can be said to exist, then the question arises as to whether modern psychology ought not to take into consideration its data, as indeed it used always to do. Thus my paper might even be called "What Became of Introspection?" One common answer to that question would be that introspection was not viable and so gradually became extinct. Another answer, however, is that introspection is still with us, doing its business under various aliases, of which *verbal report* is one. The former statement about the failure of introspection is approximately true of that introspection which flourished under Titchener at Cornell in 1900-1920, whereas the latter statement about camouflaged introspection is accepted by the modern positivists who hold that the concept of conscious experience has meaning only when it is defined operationally.

DUALISM

The belief in the existence of conscious mind in man is very old, as old as philosophy and as old as the belief in the immortality of the soul, the immortality of that part of a person that is not his mortal body. Thus it has come about that something conscious is usually one term in a dual-

ism, like mind *vs.* matter, the rational *vs.* the irrational, or purpose *vs.* mechanism. There have been psychological monists, like La Mettrie (44), the materialist, who argued in 1748 that man is a machine and who got himself consequently into theological trouble, but even he was more concerned with reducing to their bodily bases the mental states that dualism had already established than in describing man without benefit of dualism.

Inevitably the doctrine of immortality and the old-time importance of theology played a role in psychology. The words for soul and mind are not distinguished in French and German (*l'âme, Seele*) nor are the Greek and Latin words (*psyche, nous; anima, mens*) as distinct as the English translations. It was the faculty of reason that carried with it the right to immortality, and Descartes, a devout Catholic, gave men rational souls, made of unextended immortal substance, and maintained that animals are mortal irrational automata (20). Thus Descartes became an important ancestor in both the dualistic (conscious, introspective) line of descent, and in the objective (mechanistic, reflex, tropistic) line.

British empiricism fixed dualism and the concept of consciousness upon psychology. Locke, Berkeley, Hume, Hartley, Reid, Stewart, Thomas Brown, the two Mills, and

Bain, all were concerned in different ways with how the mind gets to know about the external world. Thus they recognized the basic mind-matter dichotomy. Presently there came also into the hands of these philosophers the doctrine of association which dealt with the synthetic relations among the items of mind or consciousness (8, pp. 157-245). There never was—nor is there now—a good word for this immaterial term of the mind-matter dichotomy. James was complaining about that in 1890 (32, I, pp. 185-187). Mostly the word was either *mind* (*Seele*) or *consciousness* (*Bewusstheit*). Nineteenth-century psychology formulated the dichotomy as psychophysical parallelism, and that doctrine was so firmly impressed upon psychological thinking that the American operational revolution of the present century came about only with the greatest difficulty.

It would not be profitable to go into great detail here about the history of the belief in what we are calling *consciousness*. The existence of consciousness seemed for many centuries to be an obvious immediate datum, the basic undeniable reality of one's own existence. "Cogito, ergo sum," said Descartes. James summed the matter up (32, I, p. 185):

*Introspective Observation is what we have to rely on first and foremost and always. The word introspection needs hardly to be defined—it means, of course, looking into our own minds and reporting what we there discover. Every one agrees that we there discover states of consciousness. So far as I know, the existence of such states has never been doubted by any critic, however skeptical in other respects he may have been. That we have cogitations of some sort is the *inconcussum* in a world most of whose other facts have at some time tottered in the breath of philosophical doubt. All people unhesitatingly believe that they feel*

themselves thinking, and that they distinguish the mental state as an inward activity or passion, from all the objects with which it may cognitively deal. I regard this belief as the most fundamental of all the postulates of Psychology, and shall discard all curious inquiries about its certainty as too metaphysical for the scope of this book.

In general the philosophers, physiologists, and physicists who founded the new experimental psychology in 1850-1870—Fechner, Lotze, Helmholtz, Wundt, Hering, Mach, and their associates—were psychophysical parallelists who would have subscribed to this view of James' (8, pp. 261-356). Psychology—even the new "physiological psychology"—was essentially the study of consciousness, and its chief method was introspection. Physiology came in because these parallelists believed in "no psychosis without neurosis" (Huxley's phrase, 30, 1874) and thus could employ the apparatus of the physiological laboratory to control stimuli and to record the effects of neural events.

About introspection (*innere Wahrnehmung*) there was, however, some question. There is a long history of opinions on the manner in which the mind observes its own processes, one that begins with Aristotle and Plato and carries on to the present. Eisler has abstracted the views of eighty-four writers on the subject, from Aristotle to the beginning of the present century (21, III, pp. 1735-1742). Locke, founding empiricism, held that all ideas—that is to say, the contents of the mind—come from experience either by sensation, which provides knowledge of the external world, or by reflection, which is the inner sense and provides knowledge of the mind's own operations. Neither sensation nor reflection, however, was regarded by the early empiricists

as a process subject to error. The belief grew up that to have conscious experience is also to know that you have it, and thus ultimately Wundt, basing his new systematic physiological psychology upon British empiricism, defined introspection as immediate experience (98, pp. 1-6). The facts of physical science, he thought, are mediated and derived by inference from immediate experience, which in and of itself is immediately given and constitutes the subject matter of psychology. This view suggests that Wundt thought that introspection cannot lie, but actually there was an inconsistency there, for the Wundtian laboratory put great emphasis upon training in introspective observation and in the accurate description of consciousness.

Brentano wrote in 1874: "The phenomena inwardly apprehended are true in themselves. As they appear . . . so they are in reality. Who then can deny that in this a great superiority of psychology over the physical sciences comes to light?" (12, I, pp. 131-203). Against this view, James remarked: "If to *have* feelings or thoughts in their immediacy were enough, babies in the cradle would be psychologists, and infallible ones" (32, I, p. 189). The classical objection to the *ipso facto* adequacy of the immediate was raised by Auguste Comte, the founder of positivism, who noted that introspection, being an activity of the mind, would always find the mind introspecting and never engaged in the great variety of its other activities (17, p. 64). Actually Comte's argument was, however, much more than this quibble, which could have been answered by the statement that introspection is not a procedure but merely the recognition that knowledge, when given, exists as knowledge. Comte was complaining, as did

twentieth-century behaviorists, that introspection is unreliable, that it results in descriptions which often cannot be verified, and that in many other ways it fails of the positive character that science demands.

J. S. Mill answered Comte's quibble by asserting that introspection is a process and requires training for reliability. It is not strictly immediate, Mill thought, for it involves memory—immediate memory, perhaps; yet immediate memory is not the datum itself and comes with a chance for error in it (53, p. 64). On this whole matter, see James' excellent discussion (32, I, pp. 187-192). Mill's point is reinforced by the modern realization that it is almost impossible to distinguish between anesthesia and immediate anterograde amnesia: a man whose memory lasts only one second is so crippled in capacity for introspection as to be practically as unconscious as any reacting organism or machine.

CLASSICAL INTROSPECTION

We may regard that introspection as classical which was defined by fairly formal rules and principles and which directly emerged from the early practices in Wundt's laboratory at Leipzig. Of course, there were no immutable rules for introspection. The great men kept disagreeing with one another and changing their minds. Nevertheless there was a body of opinion which was in general shared by Wundt, by Külpe before he left Leipzig, by G. E. Müller at Göttingen, by Titchener at Cornell and by many other less important "introspectionists" who accepted the leadership of these men. Stumpf at Berlin held to less constrained principles, and Külpe's later doctrine of introspection after he had gone to Würzburg was opposed by Wundt and Titchener.

Classical introspection is the com-

mon belief that the description of consciousness reveals complexes that are constituted of patterns of sensory elements. It was against this doctrine that Külpe at Würzburg, the behaviorists under Watson and the Gestalt psychologists at Wertheimer's initiative revolted. Introspection got its *ism* because these protesting new schools needed a clear and stable contrasting background against which to exhibit their novel features. No proponent of introspection as the basic method of psychology ever called himself an *introspectionist*. Usually he called himself a *psychologist*.

Wundt, undertaking to establish the new psychology as a science, turned to chemistry for his model. This choice landed him in elementism, with associationism to provide for synthesis. The psychological atoms were thus sensations and perhaps also feelings and images. The psychological molecules were preceptions and ideas (*Vorstellungen*) and the more complex combinations (*Verbindungen*). Because Wundt changed his views from time to time about images and feelings, the sensation became the example of the sort of stuff that appears in a good description of consciousness. Thus, half a century later, we find Titchener concluding that *sensory* is the adjective that best indicates the nature of the contents of consciousness (85, pp. 259-268). In this way Wundt fixed both elementism and sensationism upon introspection, and introspectionism in the proper laboratories always yielded sensory elements because that was "good" observation. It seems reasonable to suppose that laboratory atmosphere and local cultural tradition did more to perpetuate this value than did any published admonitions about observation.

Although Wundt defined the sub-

ject matter of psychology as immediate experience (97; 98, pp. 1-6), he did distinguish introspection (*Selbstbeobachtung*) from inner perception (*innere Wahrnehmung*). Inner perception might be self-validating, but it was not science. Wundt insisted on the training of observers. Even in the reaction experiment Leipzig observers had to be trained to perform the prescribed acts in perception, apperception, cognition, discrimination, judgment, choice, and the like, and to report when consciousness deviated from what had been called for. Thus it is said that no observer who had performed less than 10,000 of these introspectively controlled reactions was suitable to provide data for published research from Wundt's laboratory. Some Americans, like Cattell, had the idea that the minds of untrained observers might also be of interest to psychology, and later a bitter little quarrel on this matter developed between Baldwin and Titchener (8, pp. 413 f., 555). For all that, Wundt's notion of what constitutes proper introspection was much more liberal than is generally supposed, for he left room in formal introspection for retrospection and for indirect report. He was much less flexible in respect of the elements and their sensory nature.

What happened next to introspection was the acceptance of the conception that physics and psychology differ from each other in points of view but not in fundamental materials. Mach in 1886 argued that experience ("sensation") is the subject matter of all the sciences (48), and Avenarius a few years later that psychology views experience as dependent upon the functioning of the nervous system (he called it the "System C") and physics as independent of the action of the nervous

system (3). Presently, after the two men had agreed that they agreed, they had great influence upon Külpe and Titchener who were both then at Leipzig. In his textbook of 1893 Külpe accepted this distinction by point of view (41, pp. 9-13), but Titchener is the person who emphasized it most. In 1910, he was saying that the data of introspection are "the sum-total of human experience considered as dependent upon the experiencing person" (79, pp. 1-25), and later he could write the formula:

Introspection = psychological
(clear experience → report),

which means that introspection is the having of clear experience under the psychological point of view and the reporting upon it also under the psychological point of view (83, pp. 1-26). Substitute physical for psychological, and you have the formula for physics. The stock example for introspection is the illusion, the case where perception differs from stimulus-object in some respect. For perception experience is regarded just as it comes, dependent upon the perceiving of the perceiving person and thus the action of his nervous system. For the physical account of the object, however, the perceiver must be abstracted from and the physicist has resort to measurement and other physical technics. Titchener held to this distinction by point of view all his life (85, pp. 259-268).

It was Külpe who split Wundt's psychological atom, analyzing sensation into its four inseparable but independently variable attributes: quality, intensity, extensity, and duration (41, pp. 30-38). Titchener later held to this view which served to tighten rather than to loosen the constraints of atomism upon introspective psychology (6, pp. 17-35).

One of the most thorough discussions of introspection was provided by the erudite G. E. Müller in 1911 (55, pp. 61-176). Müller was more liberal than Wundt and left room for all the indirect and retrospective forms of introspection. Being primarily interested in the application of introspection to memory, he distinguished, for instance, between the present recall of the past apperception of a past event and the present apperception of the present recall of a past event, an important distinction, since present apperception can be interrogated as to detail whereas past apperception has become fixed and no longer subject to exploration.

It was Titchener who placed the greatest constraints upon introspection by his requirement that the description of consciousness should exclude statements of meaning. At first Titchener had perception in mind and called the report of meanings the *stimulus-error*, insisting that trained observers by taking the psychological point of view would describe consciousness ("dependent experience") and attempt no statements about the stimulus-objects ("independent experience" as given by the point of view of physics) (5; 79, pp. 202 f.). After Külpe had claimed to find imageless (non-sensory) thoughts in the consciousnesses of judgment, action, and other thought processes, Titchener broadened his criticism to an objection against the inclusion of any meanings at all in the data of introspection (80). He was arguing that straight description (*Beschreibung, cognitio rei*) would yield the kind of sensory contents that had become standard in classical introspection, and that inferences about conscious data (*Kundgabe, cognitio circa rem*) are meanings which do not exist as do the observed sensory processes (81,

82). Thus his psychology has even been called *existential psychology*, because he believed that the meanings, occurring as inferences, lack the positive character of sensations and images, the existential data (85, p. 138).

It was never wholly true that introspection was photographic and not elaborated by inferences or meanings. Reference to typical introspective researches from Titchener's laboratory establishes this point (28, 58, 25, 64, 59, 16, 31). There was too much dependence upon retrospection. It could take twenty minutes to describe the conscious content of a second and a half and at the end of that period the observer was cudgeling his brain to recall what had actually happened more than a thousand seconds ago, relying, of course, on inference. At the Yale meeting of the APA in 1913, J. W. Baird with great enthusiasm arranged for a public demonstration of introspection with the trained observers from his laboratory at Clark, but the performance was not impressive. Introspection with inference and meaning left out as much as possible becomes a dull taxonomic account of sensory events which, since they suggest almost no functional value for the organism, are peculiarly uninteresting to the American scientific temper.

Classical introspection, it seems to me, went out of style after Titchener's death (1927) because it had demonstrated no functional use and therefore seemed dull, and also because it was unreliable. Laboratory atmosphere crept into the descriptions, and it was not possible to verify, from one laboratory to another, the introspective accounts of the consciousnesses of action, feeling, choice, and judgment. It is not surprising, therefore, that Külpe, Watson and

Wertheimer, all within a decade (1904-1913), reacted vigorously against the constraints of this idealistic but rigid pedantry.

DESCRIPTION OF THE IMPALPABLE

What came to be called *systematic experimental introspection* developed at Würzburg in 1901-1905 under Külpe's leadership (8, pp. 401-410, 433-435). Külpe, influenced like Titchener toward positivism by Mach, had gone from Leipzig to Würzburg with the conviction that experimental psychology ought to do something about thought. The new experimental psychology could handle sensation, perception and reaction, and Ebbinghaus in 1885 had added memory to its repertoire. Wundt had said that thought could not be studied experimentally, but Külpe, a positivist, was convinced that all you had to do was to get observers thinking under controlled conditions and then have them introspect upon the thought process.

There followed a brilliant series of papers by Külpe's students: Mayer and Orth on association (1901), Marbe on judgment (1901), Orth on feeling (1903), Watt on thought (1905), Ach on action and thought (1905). Every one of these investigators found what we have called classical introspection inadequate to his problem. Mayer and Orth could describe the associated trains of images that run on in thinking but could discover from introspection no clue as to how thought is directed toward a goal (50). Marbe found judgments forming readily in terms of images, but got from introspection no hint as to how or why they were formed (49). Feeling resisted Orth's introspective analysis and he was obliged to invent a vague term, *conscious attitude*, to describe the affective life. Certainly feelings did not

appear as sensations or images to his observers (60). Watt and Ach worked independently and came to mutually consistent conclusions. Watt, to make introspection more efficient, invented fractionation. He split up the psychological event under investigation into several successive periods and investigated each by itself, thus reducing the amount of memory and inference that were involved in the introspective report. Still the essential in thought eluded him, until he realized that the goal-directedness of thinking is predetermined by the task or instruction—the *Aufgabe* he called it—which the observer accepted before the individual thought process got under way (92). Ach developed the concept of the *determining tendency* as the unconscious guide which steers the conscious processes along a predetermined course to solve whatever problem thought is directed upon. He also elaborated fractionation with chronoscope control and coined the phrase *systematic experimental introspection*. The determining tendency itself is unconscious, but the conscious processes which it directs seemed to Ach's observers not to be describable in the terms of classical introspection, that is to say, in images and sensations. Ach therefore invented the term *awareness* for these vague and elusive contents of consciousness and his observers learned to describe their consciousnesses in terms of impalpable awarenesses (*unanschauliche Bewusstheiten*) (1).

The Würzburgers thought they had discovered by introspection a new kind of mental element, but the *Bewusstheit* never gained the accepted status of a sensation or an image. Instead the Würzburgers were said to have discovered imageless thought, and many persons argued that the

school had failed because its finding was negative: thoughts were not images, but what actually were they? Titchener, however, believed he knew. He said that these Würzburg thoughts were in part conscious attitudes which are vague evanescent patterns of sensations and images, and in part meanings and inferences which ought to be kept out of psychology as the *Kundgabe* which is not true description (80). We, with the perspective of forty years upon us, see that the main contribution lay in the realization of the importance of the unconscious *Aufgabe* and determining tendency. The course of thought is unconsciously determined: that is a conclusion which fitted the *Zeitgeist* of the period of its discovery, when Freud too was discovering that motivation is ordinarily not available to introspection.

Külpe's conclusion was, however, different. He believed that the impalpable awarenesses had been established as valid data of consciousness and he called them *functions* to distinguish them from the sensations and images of classical introspection, which he called contents (43). *Funktionen* and *Inhalte* are two kinds of conscious data that make up what has been termed the bipartite psychology of Külpe's later days. In this choice Külpe was combining the introspection of Wundt with the introspection of Brentano. He was also making easier the coming protest of Gestalt psychology against Wundtian introspection.

AWARENESS OF MENTAL ACTIVITY

Meanwhile nearly all the philosophers and psychologists were dualists and most of the psychologists were also psychophysical parallelists. If you believe in conscious events as dependent upon brain events but wholly separate and different from

the brain events, then you must believe in some kind of introspection or inner perception whereby you obtain your evidence about the mental events. The behavioristic monism of the twentieth century was unknown in the nineteenth. A belief in some kind of introspection was general in psychology and also in common sense.

The appeal to introspection was especially important in the case of act psychology, which claimed that a careful and unbiased examination of the mind shows that it does not consist of stable contents like images and sensations, but of acts directed intentionally upon an object or of activities striving purposively toward a goal (8, pp. 439-456, 715-721). We have already seen that Brentano defended introspection as self-validating. He was the representative of intentionalistic act psychology who was contemporary with Wundt, and who thus posed the dilemma between Wundt's contents and his own acts (12), a dilemma of which Külpe, as we have just noted, seized both horns. Brentano influenced the philosopher James Ward in his subject-object conative psychology of 1886, revised in 1918 (87), and Ward influenced McDougall, who, in spite of having once defined psychology as the science of behavior, elaborated a purposive psychology in 1923, a system that made purpose and striving a characteristic of all mental activity (51).

In Germany, Stumpf, stimulated by Brentano's sponsorship of psychic acts and by Husserl's argument for phenomenology as the simplest description of experience (29), came to the conclusion that Wundt's kind of introspection yields the data of phenomenology but that psychology proper consists rather of Brentano's acts or, as Stumpf called them, *psychic functions* (76). Thus it is cor-

rect to say that by 1915 both Stumpf and Külpe believed in two kinds of introspective data: on the one hand, Stumpf in phenomena and Külpe in contents, and, on the other, both of them in functions (acts). Külpe was inclined to think that the functions were observed retrospectively (*rückschauende Selbstbeobachtung*), the contents immediately (*anschauende Selbstbeobachtung*) (43, pp. 42-45).

Except for Titchener and his satellites, American psychology tended all along to be practical and functional in the Darwinian sense. As such it was destined to become behavioristic. It is interesting, therefore, to note that early American functional psychology of James, Dewey, Angell, and the Chicago school was introspective. Organisms have acquired consciousness because of its adaptive function, the argument ran. When the smooth course of habitual action is interrupted by external events, then "in steps consciousness," said James Angell, to solve the organism's problem (2; 9, pp. 276-278). It is because functional psychology regarded the data of consciousness as essential to an understanding of the adjustment of man to his environment that Watson, founding behaviorism, declared that he was as much against functional psychology as against introspectionism.

PHENOMENOLOGICAL DESCRIPTION

The next protest against the constraints of classical introspection came in connection with the founding of Gestalt psychology—by Wertheimer, we generally say, in his paper of 1912 on seen movement (94). Wertheimer was working on the conditions of visually perceived movement. You can see movement when no stimulus object moves, as when stimulus displacement is discrete. Seen movement is thus a con-

scious, not a physical, event. Classical introspection would have required the description of perceived movement with reference to conscious contents, or mental processes, or images and sensations, or perhaps the attributes of sensation. Wertheimer thought, however, that any such reference or analysis would be a supererogation. Perceived movement can be recognized as itself and its conditions studied; why bother then with the Leipzig hocus-pocus? Since seen movement can thus be accepted immediately as an identifiable phenomenon, Wertheimer called it Φ —the " Φ -phenomenon." In 1912 the notion of phenomenology was in the air. Husserl had used the term for the free unbiased description of experience ("being") (29) and Stumpf had picked it up (76). Thus Köhler and the other Gestalt psychologists came always to speak of the data of direct experience as *phenomena*, avoiding all the words that were associated with classical introspection. Later it was such *phenomenological observation* that became a technic to displace *introspection* (8, pp. 601-607).

This Magna Carta of phenomenology presently released a great deal of good research, most of it on problems of perceptions. In G. E. Müller's laboratory Katz's work on brightness constancy (34) had even preceded Wertheimer's, and Rubin's classical study of figure and ground (68) came soon after. There began a long series of investigations of the laws of perceived form, studies which introduced new descriptive concepts for the phenomena, like *organization* and *articulation*, and new functional concepts, like *closure*, *transposition*, and *object constancy* (8, pp. 611-614).

Nearly all these perceptual studies have been performed in an atmosphere of dualism. You try to find

the stimulus conditions or else the brain pattern that is necessary and sufficient for the perception. Wertheimer, Köhler, and Koffka have all supported the concept of *isomorphism*, the hypothesis that the field pattern of the perception corresponds topologically to the field pattern of the underlying events in the brain, and, while neither Gestalt psychology nor experimental phenomenology requires isomorphism as a basic concept, nevertheless isomorphism requires some kind of dualism, and thus the phenomena become one term in its psychophysiological correlation. Köhler's great book on *Physische Gestalten* in 1920 supported this view (36).

As Gestalt psychology waxed, classical introspection waned. Wertheimer's paper on phenomenal movement was in 1912 (94). Külpe died in 1915. Köhler worked with apes on the island of Teneriffe during World War I and applied the new phenomenological principles in the description of their psychology (35). Koffka's students were busy publishing papers on perception. Wundt died in 1920, the year that Köhler published *Physische Gestalten* (36). In 1922 Köhler went to Berlin to succeed Stumpf. The Gestalt psychologists had started a new journal devoted to their interests in 1921, *Psychologische Forschung*, and Wertheimer used its early pages to make the case against classical introspection (94). Koffka restated the case in English for Americans in 1922 (38). Titchener died in 1927. Köhler's *Gestalt Psychology* appeared in 1929 (37), and Koffka's *Principles* in 1935 (39). It is reasonable to say that phenomenological observation had won out over classical introspection by 1930.

Under Hitler's influence the Gestalt psychologists who remained pro-

ductive all came to America. There the victory of phenomenology, made easier by Titchener's death, was no great triumph, for other strong forces were operating to swing American psychology toward behavioristics. Nevertheless, phenomenology remained, not only respectable, but stimulating and useful in initial attacks upon many psychological problems, as Gibson's recent phenomenological study of the visual world shows (26). So here we come to a case where introspection, under an alias, can be said to be still practiced, provided the word *introspection* is not restricted to its Leipzig-Cornell meaning.

PATIENTS' PROTOCOLS

The emphasis which modern psychopathology places on the unconscious creates for it a complementary concern with the conscious. Thus psychoanalysis stresses the importance to therapy of bringing repressed ideas from the unconscious into consciousness. The analyst, bubbling free associations on the couch, is certainly giving the analyst information about his consciousness (*Kundgabe*) though he remain far from the use of classical introspection. When and how, we may ask, did psychopathology get itself concerned with the content of consciousness?

Nearly always the first evidence of what we now call mental disease lies in abnormal conduct, in maladaptive behavior. The abnormal person, witch or patient as the case may be, first calls attention to himself by queer or alarming conduct. The obvious symptoms that require social action, remedial or protective, are usually not reports of visions or complaints about voices, but such deviations from standard behavior as

inconvenience others. Nevertheless psychopathology, which grew up surrounded by a belief in dualism, was never primarily behavioristic. There was for it always the presumption that a witch is conscious, even though the devil might have taken possession of her will, and later that the hallucinations and delusions of the hysterical patient are conscious phenomena. Subjectivism, always implicit in these symptoms, was not very often explicit before the end of the nineteenth century.

Zilboorg's account makes it clear how the idea of mental derangement began in the conception of demoniacal possession (96, 99). For these possessed people and for the fools, except in those cases where they were honored, the therapy consisted of discipline, threats, fetters, and blows, none of which actually had much value except to relieve those who administered the punishment. Even the Renaissance, which is said to have "discovered man," did not free these unhappy victims of an intolerant theological self-assurance, until at last the reaction toward humane treatment arrived with Pinel and his successors early in the nineteenth century. During the seventeenth and eighteenth centuries you get as subjective data the reports of melancholy (sometimes ending in suicide), of passions, of deliriums ("errors of reason"), of fantasies, of cholers, humors and madness, of spleen, vapors and hysterical distempers, of love as a cause of mental disability. An incubus might be a woman's hallucination, delusion, or wish projection, or else a fiction of other people's belief about her. The reforms of the nineteenth century toward the humane treatment of the insane and the rise of the concept of mental disease (Pinel, 1801) did not

go far toward the subjectivization of psychopathology (61). Braid's theory of hypnosis, as the scientific successor to mesmerism was called, was based on suggestion as a principle, a mentalistic but not a conscious entity (11). Liébeault cured a patient of sciatic pain by hypnosis; is a patient who says he feels pain introspecting? Liébeault was a dualist, for the title of his book asserts that he was studying *l'action de la morale sur le physique*: a treatise on psychosomatic medicine in 1866 (45). Later Charcot worked out the stigmata of hysteria and thus, as he thought, of hypnosis, but most of the stigmata were not described in conscious terms, being phenomena like anesthasias, amnesias, and catatonias (15, III & IX). Kraepelin, Wundt's one-time student, whose classical system of mental diseases reached maturity about 1896, established the basic dichotomy between manic-depressive psychoses and dementia praecox (40). Thus he recognized elation, depression, and hallucinations as symptoms of mental disease, but that is a far cry from saying that his psychiatry was based on some kind of introspection.

Nevertheless this last decade of the nineteenth century was the decade for psychopathology to turn truly psychological. It marked the emergence of Janet first, and then of Freud. Janet's classical study of the symptoms of hysteria appeared in 1892 (33), and Freud's great book on the interpretation of dreams in 1900 (24). Janet's theory of hysteria in terms of dissociation and the retraction of the field of attention was a psychological theory, although not an introspective one. Freud in his association with Breuer discovered the "talking cure" out of which psychoanalysis has emerged (13). The effect of psychoanalysis upon psychiatry

has during the present century been profound. Not only has psychiatry taken over psychoanalytic concepts while rejecting the total system, but the psychiatric interview has been arranged to assay consciousness, as well as to bring to consciousness those forgotten materials whose absence constitutes a symptom of mental disorder. Nowadays the interview and the couch are used as tools for a special kind of introspection, one which inventories consciousness and seeks to bring forgotten memories up to and across the threshold of introspection.

One of the most definite claims for the use of introspection by abnormal psychology was made by Morton Prince, Janet's complement in America, long a student of dissociated and alternating personalities, and later insistent upon the simultaneous functioning of coconscious personalities (62, 63). Prince once suggested that introspections might be obtained simultaneously from two coconscious personalities, even though they had but one set of receptors and effectors between them. You might, he thought, be able to question one personality with written questions shown to the eye and get the protocols spoken by the voice, while the other personality received spoken questions by ear and replied by writing on a pad. This is a difficult form of dissociation and, when it has been tried, the protocols tend to become habituated clichés or nonsense (69); yet Prince's suggestion carries the point that patient's protocols are, after all, a kind of introspection. The operationist can, of course, translate protocols into discriminative response, for any consciousness that yields public data can be described in behavioristic terms; yet that fact does not alter the feeling of reality that

the psychopathologists have about both consciousness, got by introspection, and unconsciousness, observed by more inferential technics.

PSYCHOPHYSICS

It was the prevailing nineteenth-century dualism of mind and body, and thus of spiritualism and materialism, that led Fechner, concerned with combating materialism and in establishing a spiritualistic monism, to invent psychophysics (22). By measuring both the physical stimulus and the psychical sensation and by showing how the magnitude of the latter is dependent upon the magnitude of the former, he believed that he was bringing mind and matter into a single system of relationships. The effect of Fechner's success in devising or standardizing the classical psychophysical methods which are still in use was to support the current psychophysical parallelism—although that is not what Fechner intended. For psychophysics the stimulus was available as an independent variable. The sensations, or the relative magnitudes of two sensations, or the sense-distances between two sensations, were available to introspection and so constituted a dependent variable in the psychophysical experiment. This kind of introspection has remained scientifically useful in experimental psychology for a full century and persists in good status today, although of course operationism has the necessary formulas for transforming it into behavioristic terms.

Before Fechner the experimental attack on sensory problems was apt to be psychophysical. Investigators determined both absolute and differential thresholds. When Bouguer in 1760 measured the differential threshold for brightness, he relied on the

observer's judgment as to when a shadow on a screen becomes only just noticeable (10, pp. 51 f.). Weber's formulation of his psychophysical law in 1834 depended on the same kind of judgment (92, pp. 44–175). Sensory phenomenology was stimulated by the discovery of the law of the spinal nerve roots (1811, 1822) which showed that the sensory nerves present a set of problems of their own. Johannes Müller's doctrine of specific nerve-energies (1826, 1838) was, in a sense, psychophysics, since it distinguished between sensory quality and the property of the stimulus which arouses the quality (56, pp. 44–55; 57, II, p. v). Many of these early instances of psychophysics, especially the quantitative ones, have been discussed by Titchener (78, II, pt. ii, pp. xiii–cxvi). There is no need to labor the point that parallelism was the accepted doctrine of the century and that psychophysics consisted in the observation of correlations, many of them quantitative, between the two correlated terms of mind and body. No one doubted that you can observe mind as sensory experience.

For at least half a century (1860–1910) psychophysics flourished along with classical introspection and came under some of its constraints. It was thought, for instance, that observers need special training in order to give reliable results. Titchener, as we have already seen, warned against the stimulus-error (5; 79, pp. 202 f.), and both Wundt and Titchener believed that control stimuli (*Vexirversuche*) were improper. For instance, in determining the limen of dual impression upon the skin, you vary the separation of the esthesiometer points according to some standard procedure, but you do not throw in single points as controls—not if you

are a classical introspectionist. The control lies in training the observer to avoid the stimulus-error. If he says *two* when he has only one, he is not wrong, for introspection cannot lie—or at least it was thought that good introspection of trained observers cannot lie very much, and in any case to argue that a one-point stimulus cannot give rise to a two-point perception is to prejudge the experiment which seeks to find what it is that you do feel for every value of the stimulus.

The same point about introspection appears in Wundt's method of identical series for the investigation of recognition (66, pp. 24–30). In this method you give the observer a series of stimulus-objects, and later you give him in the test the identical series again, having him state which items he recognizes. You do not introduce new items as controls. He knows the series are the same, but you trust him in his introspection. He will not report recognition for an item unless he experiences recognition, and no one but the observer himself can publish the privacy of his own consciousness. If you place all this responsibility on the observer, no wonder training becomes important.

This kind of incontrovertible psychophysical introspection did not last long in the functional atmosphere of American psychology. Perhaps it has not now been heard of for thirty years.

For the half century after Fechner the psychophysicists always talked about observing and measuring sensation, but actually they were observing, reporting upon, and measuring, not complete sensations, but sensory attributes. From Fechner on, the psychophysical methods were applied to judgments of the quality,

intensity, extensity, or duration of sensory experience, and Külpe, after he broke away from Wundt, suggested that you never actually do observe a whole sensation, but only separately its attributes, out of which you build the sensation up as a scientific construct (42). Later Rahn, a student of Külpe's, reinforced this comment (65), and Titchener ultimately adjusted his views to meet the contention (84).

Külpe in 1893 had argued that the attributes of sensation are (a) inseparable from the sensation (if any attribute becomes zero, the whole sensation ceases to exist) but (b) independently variable with respect to each other (you can change one and keep the other constant) (41, pp. 30–38). Later this view turned out to be wrong, for there are separate attributes, like the pitch and loudness of tones and the hue and brightness of spectral lights, which cannot easily be varied independently by controlling their stimulus. Stevens solved this problem by an appeal to the concept of invariance. You have, he said, an independent attribute if it remains invariant when the dimensions of the stimulus are varied in accordance with some unique determined function (7, 70, 71). This concept results in plotting isesthetic contours on a stimulus diagram, e.g., in plotting isophonic contours for pitch and loudness against stimulus frequency and energy, or isochromatic contours for hue, brightness, and saturation against stimulus wavelength and energy. Sensory equality becomes the crucial datum, but subjective equality is computed from the same basic introspective data that Fechner used—judgments of *greater* and *less* or of some similar complementary categories.

Modern psychophysics is also en-

gaged in the determination of sensory interval scales and ratio scales, and for this purpose observers report on the relation of one sense-distance as greater or less than another (interval scale) or on the ratio of one sensory attribute to another (ratio scale) (75, pp. 23-30). Such introspection is reliable and receives general approval, even in behavioristic America.

There are other less quantitative kinds of psychophysics which still make successful use of reports on sensory experience and which can be properly classified as modern introspection. An excellent example is Crocker's work on the analysis and assessment of flavors by trained panels of judges, persons who are really introspectors especially trained to appreciate and analyze tastes and smells (18). They estimate the degree of the various olfactory and gustatory components in a flavor, check judgments against one another, working as a cooperative team with high motivation and enthusiasm. Such a trained panel may be sent out from the parent laboratory to some industrial plant to savor and calibrate its product, and then may later be brought back to the parent laboratory for checking in introspective reliability and also, when necessary, for analytic recalibration. Crocker's account of how attitudes are fixed and judgments rendered uniform in these panels is reminiscent of the atmosphere of Wundt's laboratory in all respects, except that Crocker's laboratory lacks the authoritarian control of Wundt's.

Another recent example of the modern use of the report of sensory experience is the book on pain by Hardy and his associates (27). This book sets forth the psychophysics of pain, having regard, among other things, to the different qualities of

algesic experience, and to establishing a sensory scale of pain by the subjective equation of algesic sense-distance.

The lesson to be learned from psychophysics is, therefore, that, in respect of the observation of sensory experience, introspection has thrived for a hundred years and is still in style.

ANIMAL CONSCIOUSNESS

In denying rational souls to animals, Descartes had made the problem of animal psychology relatively unimportant, but Darwin, with his evolutionary argument that the forms of both mind and body show continuous development from lower species to man (1872), changed all that (19). You began then to hear from Romanes about mental evolution and the evolution of intelligence (1883). Romanes coined the term *comparative psychology* for the study of the nature of mind in different species (67). By giving the animal mind the benefit of the doubt, he was able to represent animal intelligence as not so far below man's. Lloyd Morgan, writing a comparative psychology, sought to temper Romanes' enthusiasm with the principle of parsimony: do not interpret an action as the outcome of the exercise of a higher psychical faculty, he said, if it can be interpreted as the outcome of one that stands lower in the psychological scale (54). Lloyd Morgan warned against "anthropomorphism" in assessing animal behavior—meaning, of course, anthropopsychism. Loeb, establishing the concept of tropism and the unconscious action of lower animal forms (1890), suggested that consciousness emerges in the course of evolution as it becomes needed for more adaptive action and that the faculty of associative memory constitutes a cri-

terion of it (47). Experiments on animal intelligence began, notably Thorndike's in 1898 (77). In the decade 1900-1910 there was marked activity in experimental comparative psychology, a great deal of it concerned with the measurement of animal intelligence for which the maze was regarded as a very useful instrument.

Although there had already been argument put forward in favor of an objective animal psychology (4), comparative psychology got under way in a period when a psychology with consciousness left out was generally regarded as psychology without its psyche—a branch of physiology perhaps. American functional psychology kept consciousness inside the fold, and the comparative psychologists settled on a formula for the observation of animal consciousness which might well have been called *animal introspection*. Nowhere has this problem been more clearly stated than by Washburn in her handbook of 1908 on the animal mind (88, p. 13). She wrote:

If an animal behaves in a certain manner, what may we conclude the consciousness accompanying its behavior to be like? . . . At the outset of our discussion . . . we are obliged to acknowledge that *all psychic interpretation of animal behavior must be on the analogy of human experience*. We do not know the meaning of such terms as perception, pleasure, fear, anger, visual sensation, etc., except as these processes form a part of the contents of our own minds. Whether we will or no, we must be anthropomorphic in the notions we form of what takes place in the mind of an animal.

There is an implication here that you learn about human consciousness by direct observation of it in introspection, but that animal consciousness is known only indirectly by analogical inference. Not every-

one held to that difference, however. Max Meyer put forward what he called the psychology of "the other one," an argument that your own personal consciousness is not material for science, being particular and not general, and that psychology studies always other organisms—other people, other animals (52). In this sense both the animal's conduct and man's words are introspection if they are taken as meaning something about the subject's consciousness. Even Titchener can be found saying of this argument from analogy: "The animal is thus made, so to say, to observe, to introspect; it attends to certain stimuli, and registers its experience by gesture" (79, pp. 30-36).

It is interesting to see how Watson, before he had thought out behaviorism, accepted the current belief of this first decade of experimental animal psychology that knowledge of animal consciousness is the ultimate goal in comparative psychology. Watson was still at Chicago, the home of systematic functional psychology, which held that consciousness is to be understood psychologically in terms of its use to the organism. He had entitled his monograph of 1907: *Kinaesthetic and Organic Sensations: Their Role in the Reactions of the White Rat to the Maze* (89, pp. 90-97). In this investigation he eliminated vision, hearing, taste, smell, and certain cutaneous factors from the repertoire of the rat who still remembers how to run the maze, and he concluded that "intra-organic sensations—the kinaesthetic sensations coupled with the organic probably, and possibly with the static" are what the rat uses in following the correct path. Watson even discussed the possibility of the rat's use of visual imagery, which "in our own case would play a preponderating role." He suggested that success for

the rat as it runs may reassure it: "If the turn is made at the proper stage (and it has been shown that blind rats deprived of their vibrissae can make these turns without allowing their bodies to touch the edges of the openings at the turns), the animal may be supposed thereby to get a 'reassuring feeling' which is exactly comparable to the experience which we get when we touch a familiar object in the dark."

Later, of course, Watson repudiated this supererogatory concern with consciousness and asked psychologists to get "closer to their data of stimuli and responses. That was a move toward positivism, but Watson did not think of that. Indeed, it is possible to regard animal behavior as a kind of language which means something about consciousness, just as it is also possible to strip introspection of its meanings and regard it as mere verbal motion. Certainly, if Max Meyer's "other one" can introspect, the animals can too and did before behaviorism made their consciousnesses unimportant.

VERBAL REPORT

Watson's reaction in 1913, away from the pedantry and unreliability of introspection, as he saw it, toward the more positive psychology of stimulus and response, was an attempt, not so much to create behaviorism as a new psychology with consciousness left out, as it was to reformulate the old psychology in new terms (90). For the imagery of thinking, he suggested that we can substitute incipient subvocal movement. Feeling, he believed, might turn out to be endocrine. Association had already been shown by Pavlov so be a conditioning of reflex responses and not necessarily a connection among ideas. Watson formally ruled introspection out of psy-

chology but he left in the more reliable results of introspection, notably in psychophysics (91). Thus it was necessary for him to leave in introspection as verbal report. Did he thus embrace the bath with the baby? Is introspection anything more than verbal report?

Actually there is a difference. Verbal report viewed simply as behavior is capable of physical specification, in which the writing and speaking of words appear as very different kinds of movements until they have been shown to be equivalent in an experimental situation. On the other hand, verbal report as introspection is not response but observation and description and therefore reference, an indication of objects of observation in the sense of the meanings of the words used.

Another way of expressing this same matter is to write two formulas:

[1] Introspective observation:

$E \rightarrow O = S \rightarrow \text{facts of consciousness}$

[2] Behavioristic observation:

$O = E \rightarrow S \rightarrow \text{facts of psychology}$

The corresponding sentences are: [1] In introspective observation, the experimenter notes the facts of consciousness which the observer, who is the subject, has observed. [2] In behavioristic observation, the observer, who is the experimenter, observes the behavior of the subject in respect of its implications for the facts of psychology. In classical introspection the subject is the observer. He has responsibility for the correctness of his descriptions of conscious data and thus he had at Leipzig, Cornell, and elsewhere to be trained, for introspection is more than having experience. Behaviorism shifts the locus of scientific responsibility from an observing subject to the experimenter who becomes the observer of the subject. In this way it is possible to bring to psychological

observation irresponsible and untrained subjects—animals, children, the feeble-minded, the mentally ill, and also the untrained normal human adult. Thus all the mental tests come into psychology because mostly they involve verbal responses from naive subjects. And the animal experiments come in because ordinarily the discriminative behavior of the animals is a language devised by the experimenter and taught to the animal so that he can tell the experimenter about his abilities and capacities. Are we to say that the animal is not introspecting because he is not communicating to himself what he is communicating to the experimenter? Perhaps. The important thing is to see that Watson, in attacking introspection, was objecting, not to the use of words by the subject, but to trusting the subject to use the words only with those meanings that the experimenter wishes the words to have.

INTROSPECTION AS AN OPERATION

Watson, in substituting verbal report for introspection, was moving in the positivistic direction, but the culmination of this movement came later with the acceptance of operational definitions as providing the most secure specification for psychological concepts. Operationism is perhaps a movement toward greater precision in scientific thinking, but it is not a school. American psychologists first picked up this modern form of the old positivism from the physicist, P. W. Bridgman, who was using the technic to explain relativity theory (14). Then it was found that logical positivism, as the movement came to be called later, was developing at the same time among the logicians in Vienna (23, pp. 1-52). Presently it became clear that the two movements were logically the

same. Stevens undertook to be the expositor to American psychologists (74). Bridgman was content to let operational definition go back ultimately to experience, but for psychologists that regression would not do at all. For them experience was a concept in special need of definition, since the availability of consciousness to scientific observation was the main problem dividing the schools (72, 73). The effect of a great deal of discussion along these lines in the 1930's was a change in the status of consciousness from (a) the reservoir of experience upon which all empirical science draws to (b) a concept based upon observation and specified by the observational operations that make conscious data available to science. That is a large change from the introspection that cannot lie because the having of experience is the knowing that you have it.

Nowadays the word *introspection* has dropped out of use. *Consciousness* or *phenomenal experience* or *sensory datum* or some other equivalent mentalistic term indicates a psychological construct which is got by inference *from* the observations. A comparable concept is the *intervening variable*, and a case could be made for Tolman as a phenomenological operationist, directly observing purpose and kindred entities in his data. Do you truly observe consciousness or an intervening variable? Do you observe any construct, or do you infer it? Do you look at the ammeter and observe the strength of the current or is what you observe merely a pointer on a scale?

Thus the answer to the question "What became of introspection?" seems to be this. Introspection as a special technic has gone. The object of introspection—sometimes called *consciousness*, sometimes something else—is a construct like an ability, or

an intervening variable, or a conditioned response, or any of the other "realities" out of which a general psychology is formed. The modern equivalent of introspection persists in the reports of sensory experience in psychophysics, in the protocols of patients with psychological difficulties, in the phenomenological descriptions of perception and other psychological events as provided notably by Gestalt psychologists, and also in a great deal of social psychology and psychological philosophy where the Cartesian dualism is still found to be convenient.

UNCONSCIOUSNESS

Any study of the history of the availability of consciousness to scientific observation, like the present one, gains significance as we consider also the availability of unconsciousness to science. *A* is specified clearly only with respect to *not-A*. It would not, however, be proper to undertake now the consideration of all the means whereby a knowledge of unconscious psychological events has been brought into science. Nevertheless we may use a paragraph to list the outstanding fields which contributed to what nowadays we call psychology and which got along, nevertheless, without any observation that might be called *introspection*.

The *reflex* was thought almost from its discovery to be unconscious, largely because it could occur without the brain, although Pflüger was of the opinion that its purposiveness implies that it is conscious. Was Lotze, who disagreed with Pflüger, relying on introspection to be sure that reflexes are unconscious? *Instinct* was ordinarily opposed to intelligent action and often supposed to be unconscious. Unconsciousness, however, was not ordinarily involved

in its definition; the criterion for instinct was that it was unlearned and usually involuntary. Loeb's *tropism* was defined with consciousness irrelevant. Herbart's *ideas in a state of tendency* were defined as unconscious, as were Fechner's *negative sensations*. Although the Würzburg school was developing systematic introspection, it seems clear now that its great discovery was the existence and effectiveness of unconscious tendencies—the *determining tendency*, the *Aufgabe*, etc. Freud made the concept of the unconscious familiar to everyone and also started the development of the technics of observation that now replace introspection, but the test of unconsciousness (suppression, repression) remained in part introspection, the fact that ideas that might have been expected to be in mind were conspicuously absent. Thus dynamic psychology carries on with the basic assumption that you cannot trust the subject's personal belief (introspection) for the true assessment of his motives.

In all these cases consciousness is seen to have been important in a negative manner, for its absence is a matter of interest and sometimes even an essential specification—as would, indeed, be expected in a psychology that was originally formed on the dualistic pattern. Indeed it is only in a dualism that *consciousness* has a distinctive meaning.

CONCLUSION

Now let the writer say what he thinks has become of introspection.

There have been in the history of science two important dichotomies that have been made with respect to introspection. (a) The first is animal psychology vs. human psychology: human beings are supposed to be able to introspect, and animals are not. (b) The second is the unconscious

mind *vs.* the conscious mind, with introspection the means of observing consciousness. These two dichotomies reduce, however, to one: inference *vs.* direct experience.

Operational logic, in my opinion, now fuses this single dichotomy because it shows that human consciousness is an inferred construct, a concept as inferential as any of the other psychologists' realities (32, p. 184), and that literally immediate observation, the introspection that cannot lie, does not exist. All observation is a process that takes some time and is subject to error in the course of its occurrence.

Introspection's product, consciousness, appears now in the bodies of its progeny: the sensory experience of psychophysics, the phenomenal data of Gestalt psychology, the symbolic processes and intervening variables employed by various behaviorists. the ideas, the manifest wishes, the hallucinations, delusions, and emo-

tions of patients and neurotic subjects, and the many mentalistic concepts which social psychology uses. The newest usage is this latter one, social perception, a term which refers both to the perception of social phenomena, like anger and danger, and the perceptions which are understood by reference to their social determinants; but here the introspection is not different in kind from the phenomenological description that the Gestalt psychologists still use.

In general, however, it seems to the writer that there is no longer to be found any sharp dichotomy setting off the introspectable from the unconscious. That once fundamental distinction disappeared with the dissolution of dualism. Consciousness nowadays is simply one of many concepts which psychology employs, usually under some other name, whenever it finds the category useful for the generalization of observations.

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A BRIEF CRITICAL REVIEW OF LOUDNESS RECRUITMENT

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The growth of loudness in the normal ear when the energy of a pure tone is increased above the absolute intensive threshold has been well specified by workers too numerous to mention here. In less detail, the growth of loudness in the normal ear has been studied in the cases of speech and of noise. For the ear which is not normal, however, there are a variety of aberrations in loudness which may occur during an increase in signal strength. Chief of these is that, for an equivalent increase in signal strength, the growth of loudness in a non-normal ear may outstrip by far the growth of loudness in a normal ear. By the time a tone is raised 100 db over normal threshold, a partial deafness of up to 50 or 60 db may be rendered negligible. This paradox, revealed by a variety of experimental and clinical techniques, was named "recruitment" by Fowler. The term is now commonly used in this country, though the terms regression, recuperation, *lautstärkeausgleich* (i.e., loudness-compensation), and Fowler-phenomenon are more common in other countries.

This general phenomenon which has its behavioral manifestations in a wide variety of psychoacoustic situations has importance for studies of the physiology and neurology of loudness. It is primarily, however, a psychological manifestation, and it is rather surprising that to date no original article has appeared on this topic in the psychological journals.

The practical importance of recruitment is rapidly claiming the attention of a great number of clinics. Audiometer manufacturers in this country and abroad are designing and selling equipment specifically for the purpose of studying recruitment. It is now widely used as an aid in fixing the locus of an auditory impairment as between the middle and the inner ear, and has even been suggested as further distinguishing the locus as between the organ of Corti and the auditory nerve.

Theoretically, the importance of recruitment lies in what it can tell us of the nature of loudness in the normal-hearing ear. It is now clear that the neurological explanation of recruitment advanced by Lorente de Nò (23) and the explanation in terms of constant loudness-loss (112) (both views receiving the widest circulation) do not account for the clinical data, and so can contribute nothing to the understanding of normal loudness phenomena. Yet, if we understood how, in recruiting ears, the loudness function can take a variety of quite different shapes, we would better understand the normal case. The experimental facts, not always consistent among themselves, are now quite voluminous, and a critical review of recruitment seems appropriate at this time before the now rapidly oncoming avalanche of clinical data gives an illusion of complete knowledge, burying any attempt to arrive at a more fundamental knowledge of the phenomenon.

HISTORY

Haberman (40) was among the first of modern otologists to note that certain individuals understood the spoken better than the whispered voice. This is true in some recruiting ears. Gradenigo (35) devised the phrase *Index Vocalis* for the quotient of the distance at which the whispered voice is just audible, divided by the same measure for the spoken voice. In a recruiting ear the distance for the spoken voice is relatively large, and the *Index Vocalis* is smaller. The *Index Vocalis* has been determined for the normal ear for a variety of speech sounds by Gradenigo, Zwaardemaker (128, 129), and Hiddema (49). Veis (121) first applied the Index to clinical material, finding a smaller value than normal with some types of hearing loss.

Pohlman and Kranz (100) first used pure tones rather than speech in recruiting ears. They found that partial tonal gaps in the audiogram seemed to disappear at suprathreshold intensities. The method they used was a forerunner of the quantitative method of monaural loudness balancing between two frequencies, to one of which the ear is relatively insensitive.

The clinical possibilities of recruitment were first clearly seen by Fowler (21, 22, 23, 24, 25, 26, 27, 28, 29, 30), who devised a number of techniques for collecting and reporting the data. As a result largely of his articles, every up-to-date otologist has by now added to his armamentarium some test for recruitment. Fowler's general conclusion was that recruitment is an index of nerve deafness, and forms a valuable tool to diagnose that condition.

Katz and Salis (64) first carefully studied the shape of the speech artic-

ulation function in the recruiting ear. From later extensions of this important work it is possible to attack the question of what the fundamental auditory abilities are which contribute to understanding speech. Thus, the garbling of speech by a recruiting ear may perhaps eventually be explained partly by a shift in pitch cues, partly by a shift in intensity cues, and partly by a change in the temporal pattern of the loudness of steady-state sounds.

The behavior in noise of normal and of recruiting ears was investigated by Langenbeck (68, 70), who showed distinct differential effects. These results have been the basis for a wide variety of tests of masking as it affects reception for pure tones and for speech (7, 8, 36, 37, 38, 39, 56, 61, 62, 93, 123, 125, 126).

Bustamente Gurria and Garibay (9) made a significant contribution when they separated disorders of the organ of Corti from deafness due to a failure of auditory nerve conduction. They point out that deafness due to impossibility of movement of endolymph, in certain conditions of the labyrinthine capsule, should theoretically be classed as conduction deafness, since it is for mechanical reasons that the organ of Corti cannot function properly.

De Bruïne-Altes (7) published the most extensive monograph to date on recruitment. She presented full case histories on 47 partially deaf patients, with complete data on recruitment collected by whichever techniques were appropriate for the particular patient. A wide variety of types of deafness was included, and the monograph is a storehouse of valuable information. Unfortunately, no patients were available with tumor of the VIII nerve. This theoretically very important group would

have added much to de Bruïne-Altes' thinking.

Dix, *et al.* (15) studied 20 cases of verified degeneration of the auditory nervous system, most of which showed no recruitment at all. This study and later work (17, 106) showed the need to re-evaluate the general agreement with Fowler's and Lorente de Nò's conclusion that recruitment must be "pathognomonic" of nerve involvement. It may be confined to non-neural structures in the cochlea (80, 96, 127).

The first indication that the condition underlying recruitment might affect other psychoacoustic relationships besides the loudness function (and related phenomena), was given by Lüscher and Zwislocki (83), who showed that in a recruiting ear it took longer, in milliseconds, for a pure tone to attain a "pitch-quality." Since then, several other facts have appeared relating recruitment to other nonloudness phenomena (12, 48, 74, 91, 109).

Our present view, then, is that in recruitment we have an aberration of loudness which has no good explanation (though it can indeed be ascribed to the sense organ rather than to the nerve), which intrudes into many aspects of hearing (and not only those aspects associated with loudness), and which is extremely valuable in the clinic as a diagnostic sign.

PREVIOUS REVIEWS

Important general discussions of recruitment are given (33, 43, 45, 50, 96, 114, 119), especially of its clinical significance (7, 9, 16, 17, 22, 23, 30, 31, 55, 56, 57, 59, 78, 82, 84, 122, 123, 127). Fairly complete instructions for determining the presence of recruitment by two or more methods are provided (7, 13, 22,

31, 62, 65, 81, 88, 102, 120, 122, 123).

PSYCHOACOUSTIC SITUATIONS AFFECTED BY RECRUITMENT

In general, the presence of recruitment in an ear will affect all those psychoacoustic relationships connected with loudness, but it is also known to affect such relationships as masking, fatigue, pitch perception, and the intelligibility of speech.

The Loudness Function

Recruitment, defined as an abnormally rapid growth of loudness, should perhaps best be determined in the suspected ear by actually deriving the loudness function for that ear from the usual processes of fractionation, bisection, etc. Surprisingly enough, no one has published a loudness function, derived in this fundamental way, for a partially deafened ear. This is probably because those individuals who contributed to the definition of the normal loudness function, mostly classical psychologists and communications engineers, have not usually had a corresponding interest in clinical deafness.

It would be instructive to derive the loudness function by the usual means in a series of partially deafened ears, and to compare individual cases with the normal function. The objection could be raised that the comparison is impossible because there would be no way to equate a loudness of one sone between two quite different individuals. This is true. However, something could be done by studying the slope of the loudness function, or by selecting individuals with one normal ear and one ear partially deafened and recruiting. Loudness functions drawn from these two ears could be drawn on the same coordinates, the two curves coinciding at some point, say at one sone,

found by loudness balancing to be equally loud.

A short-cut substitute for the rather laborious task of drawing independent loudness functions for the two ears of a monaurally, partially deaf patient was proposed by Fowler (21) and followed by practically all subsequent workers in this field. It was assumed that the one ear would produce a normal loudness function if the usual procedures were applied; the suspected ear could then be matched to it at a succession of loudnesses. Because by far the greater part of this work has been done with the clinical audiometer, the unit used has been Sensation Level (in db over normal threshold on the audiometer). What is actually found, then, is difference in db between threshold for the two ears, and the lessening of this difference at equal loudness at higher sensation levels. It is of course possible to re-draw the data in true loudness units, converting sensation level to loudness level (114, p. 123), and thence to loudness in sones (34). But in practice this is never done.

It is important to emphasize that it is possible to interpret a loudness function from a recruiting ear only if a perfectly normal ear exists for comparison. Much of the work on ramifications of recruitment has resulted from a search for an index of recruitment where neither ear can be considered normal.

Isophonic Contours

Where it is difficult to interpret a loudness function from a suspected ear because the other ear is not normal for that frequency, it is sometimes possible to confine one's efforts solely to the suspected ear if that ear is normal for *some* frequency. Monaural loudness matches can then be

obtained between the two frequencies, and an interpretable loudness function drawn for the suspected ear. This has never been reported, but again a short-cut in terms of sensation level has been proposed (7) and widely used. A modification, the drawing of isophonic contours in a suspected ear, has been extensively used (11, 70, 71, 100, 104, 110) by comparing the contours with the normal case (19). It often happens in a partially deafened but recruiting ear that at threshold (isophonic contour = 0) quite a loss in loudness is seen over some range, but that at higher intensities the contours do not differ appreciably from the normal. A full set of isophonic contours is, indeed, about the only feasible way to locate the presence and extent of recruitment over the whole auditory area.

Restriction of Useful Intensity Range

As is well known, the threshold of discomfort for pure tones is about 120 db sound pressure level (SPL). Yet this threshold may not change for a partially deafened but recruiting ear. Thus, if a recruiting deafness of 60 db exists, that ear may have a dynamic range of only 60 db between audibility and discomfort. The striking difference between this and the normal case is apparent.

Intensity Discrimination

As a corollary of crowding several hundred thousand millisones into a 60-db span, as explained above, each db should encompass more millisones than normally, and intensity discrimination should improve. An improved intensive DL was proposed as an index of recruitment (6, 22, 89) and has been used in several clinics (14, 75, 80, 82, 84, 85, 86, 88, 97, 98, 99). A difference may exist be-

tween data gathered by the method of constant stimulus differences (14, 75) and by amplitude modulation (80, 82, 84, 85, 86, 88, 97, 98, 99).

Precision of Absolute Intensive Limen

As another corollary of a restricted dynamic intensity range, each db at or near absolute threshold should contribute more to the clarity of the distinction between "tone present" and "tone absent." Fowler (23) proposed the interval of uncertainty of the psychometric function, as an index of recruitment, but only a rough approximation is in use with audiometers which automatically record the db range through which the patient continuously hunts his threshold (2, 3, 4, 5, 42, 107, 108). This range is often miscalled an intensive DL.

Auditory Fatigue

The question of the relationship between recruitment and auditory fatigue, as opened up by de Maré (89), has proved very fruitful in both areas. Assessing the fatigability of an ear has been proposed for differentiating perceptive-deaf (and therefore, presumably, recruiting) ears (32, 54, 58, 87, 90). On the other hand, the presence of recruitment in a fatigued ear (1, 10, 44, 45, 72, 73) may give an idea of the locus of the fatigue effect within the auditory system (46).

It has been supposed (87, 91) that in a recruiting ear a stimulating tone has a more far-reaching effect up and down the basilar membrane. In these experiments, however, loudness of stimulation was not equated between the normal and the recruiting ear.

Masking

The presence of recruitment in-

creases the masking effect of pure tones (7, 8, 36, 37, 38, 39) and also of noise (56, 61, 69, 92, 93, 125, 126).

Speech Reception

The relation between hearing for weak vs. loud speech is discussed (7, 16, 17, 22, 27, 28, 29, 31, 44, 45, 48, 59, 60, 64, 95, 112, 113, 124, 127). A distinction must be made between the loudness of speech and its intelligibility (31, 43, 45, 101, 113). The shape of the speech articulation curve in recruitment is often quite different from normal (11, 16, 17, 22, 27, 28, 29, 45, 51, 59, 60, 63, 64, 113, 124). Two possible explanations are given (27, 51).

De Maré (91) has suggested that pitch discrimination may be affected by recruitment and may form the basis of a clinical test. Two workers (48, 109) have taken this suggestion.

Pupillary dilation upon high-frequency stimulation was thought to be accentuated in recruitment (74).

In a recruiting ear a longer time than usual is necessary for a short tone to achieve a true tonal quality (83).

CHARACTERISTICS OF RECRUITMENT

Inducing Conditions

Reports that "nerve deafness" or "perceptive deafness" is accompanied by recruitment are too numerous to cite, but we now understand that these terms are too general, early workers using them indiscriminately to refer to any deafness localized in the cochlea. There are, however, a number of case histories in the literature from which we may ascertain rather specifically the cochlear conditions giving rise to recruitment.

Boilermaker's disease, which is always accompanied by recruitment (40), is now understood to be the

result of acoustic trauma whose first manifestation is the bruising of the organ of Corti, and its final sloughing off from the basilar membrane into the endolymph of the scala media. Many cases of Ménière's disease have been proved to have no nerve involvement, the effect residing solely in the sense organ (7, 12, 15, 17, 20, 47, 80, 82), and almost all of these cases exhibit strong recruitment. All 38 cases of "traumatic perceptive deafness," and all 16 cases of "progressive hereditary perceptive deafness" in Bruïne-Altes' series (7) exhibited recruitment. On the other hand, all cochlear damage need not yield recruitment. It is absent in presbycusis (7, 83, 91) and streptomycin damage (80), and may be absent in basal skull fracture (7). We have seen that true nerve deafness does not as a rule produce recruitment (7, 15, 16, 17, 80, 82, 105, 106, 127). Finally, psychogenic deafness may produce the opposite of recruitment (79), namely, an *increase* in the intensity DL by a factor of 2-3 over the normal.

In general, the facts seem to be consistent with the view of Mygind (96), who argues that recruitment is the result of conductive impairment within the cochlea. It is clear, of course, that a variety of types of impairment can occur, and we may perhaps expect recruitment data to show variations according to type of impairment.

Shapes of Recruitment Curve

Practically all our information concerning the precise shape of the loudness function in recruiting ears comes from studies of binaural loudness balancing, one ear being normal. Data have generally been plotted in terms of audiometer units rather than in loudness units, the ordinates

representing db above normal threshold. It is clear that with two normal ears, a plot of sensation levels to achieve equal loudness will be a 45° diagonal. If, however, one ear is defective 20 db at threshold, but recruits completely within 30 db, then the plots will fall *off* the diagonal by 20 db at zero loudness, but will fall *on* the diagonal at the 50 db point. The question for the moment is, what is the exact shape of the curve by which we pass from 20 db *off* the diagonal, to the diagonal. Reger (103, 104) found the slope to increase with extent of deafness, and that the slope was steepest just above the deafened threshold. Steinberg (110) reasoned that the curve should approach the diagonal asymptotically. In general, the shape of the recruitment curve has received less attention than it deserves. Some types of disorders, e.g., those due to long-lasting auditory fatigue, produce plots on a straight line between the point for zero loudness and the point on the diagonal for complete recruitment, and others, e.g., Ménière's disease and simulated deafness by noise-masking, produce an asymptotic curve (45). It sometimes happens that a curve lies asymptotic not to the 45° diagonal, but to a line parallel with the diagonal but displaced some decibels. It has been supposed that this displacement represents a component of conductive (i.e., nonrecruiting) deafness (112, 113). However, if a conductive component is artificially added to an asymptotically recruiting ear by using an earplug, the conductive impairment manifests itself in an initial "delay" *before* the curve tends toward the diagonal. Again, this "delay" is commonly seen as a parallelism extending through 10-15 db before the curve approaches the diagonal in some cases

of inner-ear deafness with no conductive component (the comparison ear being perfectly normal), and in some cases of inner-ear deafness simulated by long-lasting auditory fatigue (never in simulated deafness by noise masking).

Considerable data must still be gathered in an attempt to discover just what conditions within the cochlea will yield certain types of recruitment curve.

Temporal Pattern

An important aspect of recruitment is that it may operate only during the first half-minute or so after stimulus onset. By using simultaneous binaural loudness balance, Hood (52, 53) and Hallpike and Hood (41) have charted a marked decline in loudness for a recruiting ear, beginning at stimulus onset and reaching a stable level only after a minute or less. A normal or a conductive-deaf ear shows no such decline in loudness under comparable conditions. This effect can completely change the interpretation of an ear from recruiting to nonrecruiting. Hood thinks it likely that here is further evidence that recruitment is confined to hair cells, and specifically their on-effect.

It is possible that another expression of the on-effect of Hood is found in the observation by Kobrak (65, 66, 67) that in perceptive deafness the patient experiences a "shock" at the initial attack of a tuning fork.

Overrecruitment

It is amazing enough that complete recruitment appears, such that a threshold deficit in one ear may be completely overcome at some higher intensity, but we find in some cases that when still higher intensities are explored, the initial deficit is more

than overcome—the same physical intensity sounds appreciably louder in the defective than in the normal ear (7, 12, 15, 17, 18, 23, 47). There is, however, a limit to which overrecruitment goes—even at very high intensities it usually amounts to only a few db. No full explanation has ever been given for the phenomenon (Lorente de Nò's explanation of overrecruitment in terms of central inhibition is the only guess attempted to date) though undoubtedly one would be forthcoming if we had a good explanation of recruitment itself; there is, after all, no reason why an abnormally rapid growth of loudness should cease just when equality with a normal ear is achieved. The same compression of loudness units could as well go on at even high intensities.

THEORIES

In the absence of knowledge of just what derangements within the cochlea induce recruitment, and of just how these derangements affect loudness, a number of theories have been offered to account for the facts.

The Fiber-Loss Theory

Steinberg's (110) explanation of recruitment was that if a few nerve fibers were defective this would have a smaller and smaller effect as the stimulus intensity increased. This explanation makes the explicit assumption that loudness is some function of number of active fibers.

The Occlusion Theory

In a discussion of a paper by Fowler (23) Lorente de Nò presented an explanation of recruitment: "If a number of hair cells in the ear or a number of fibers in the cochlear nerve is missing, the tones will appear to be weaker in intensity when near-threshold stimuli are used; but if the

intensity of the tone is increased, the more strongly activated hair cells or cochlear fibers will be sufficient to saturate, i.e., to excite the limiting intensity of the cochlear fiber or the cells of the cochlear nuclei, so that the cerebral cortex will receive the same number of impulses per second for both ears and will perceive the tone delivered to the diseased ear as strongly as the tone delivered to the normal or less affected ear" (p. 220).

It will be noted that this theory applies to both hair cell and nerve cell damage, and as such is not affected by later data on recruitment in nerve deafness.

The Constant Loudness-Loss Theory

Steinberg and Gardner (112) and Steinberg (111) attempted an explanation in psychological terms. Suppose an ear to be 20 db deaf for a certain frequency. A tone at threshold for this particular ear amounts to a loudness of about 100 loudness units for the normal ear. But suppose the tone were increased 20 db over the deafened threshold. A tone of this intensity amounts to about 1,000 loudness units for the normal ear. A further increase of 20 db intensity produces 4,500 loudness units, and so on. It is easily seen that because of the nature of the relationship between sensation level and loudness, an ear with a type of deafness resulting in a constant loudness loss would tend to overcome this handicap at high intensities, where the *per cent* loudness loss could be unnoticeable. (In the above illustration, at 40 db sensation level per cent loudness loss is only about 2.0.) This *constant loudness-loss theory* has been given wide currency (18, 50, 114).

However, when the theory is put to a rigorous test (45) it appears quite inadequate. Most cases of

asymptotic and straight-line recruitment exhibit a *continuous increase* in loudness loss, rather than a constancy. When the facts were ascertained on a patient from Dr. Fowler's practice (22), a patient often used to buttress the constant loudness-loss theory, it was found that the loudness loss at threshold (4,350 millisonnes) had risen within 10 db to a loss of 6,975 millisonnes, and within another 10 db to a loss of 11,045 millisonnes (44).

The Duplicity Theory

Lurie (77) explained recruitment in terms of the difference in sensitivity of the outer and inner hair cells. If the more sensitive outer hair cells are defective, then the threshold would be raised; but if the sound intensity were raised sufficiently to stimulate the inner hair cells, these would respond normally and a rather sudden increase of loudness might result. This explanation makes the implicit assumption, however, that in the normal ear the outer hair cells no longer contribute significantly to loudness at intensities which stimulate the inner hair cells. It is fairly clear that such is not the case. However, the division of effort among the inner and outer hair cells rests upon good histological (94) and other evidence (77). If these lines of thought are correct, it would indeed be surprising if no aberrations of loudness resulted from some condition which reduces the output of the outer hair cells (as some acoustic trauma seems certain to do) and restricts functioning to the more simply innervated inner hair cells.

The Geometric Theory

Tumarkin (117, 118) proposed that if only the more sensitive hair cells are damaged, then at some higher

intensity the inner hair cells will begin to function, and to these units the brain assigns a certain loudness. So far he follows Lurie's notions. But it is assumed that a sound, which has a certain psychological loudness to a normal ear, has a characteristic geometrical pattern of activity on the basilar membrane. Now if some hair cells in that ear are damaged, the resultant pattern for that sound will be affected, but the missing features will be "filled in" by the "memory," and the psychological contribution to loudness will override the physical deficiency. While this theory is not easily open to experimental test, Tumarkin feels supported by recent evidence that recruitment may be confined to the organ of Corti.

The Impedance Theory

Veckmans (119, 120) attempts an explanation of recruitment in physical terms. Substituting in Ohm's Law, loudness = energy/impedance. Now if the impedance changes in a particular ear as a function of sound intensity, then the usual relation between loudness and energy would be changed, and recruitment might result. Veckmans points out that impedance does change (as a function of frequency) in the normal ear, so that loudness can be said to recruit at 100 cps as compared with, for example, 1,000 cps. And he insists, without experimental evidence, that if an abnormal condition existed which caused impedance to increase with increased energy, then recruitment could be expected. The impedance theory, moreover, is contradicted by the facts of loudness balance between air and bone conduction on the same ear (45). Being of greater impedance, the bony channel should exhibit recruitment, but does not.

The Microphonic Theory

Dix, *et al.* (15) gives an explanation by analogy, referring to the case of a carbon microphone defective for low intensities but satisfactory for high. This analogy advances our thinking only very slightly.

Any theory of recruitment must meet three demands: it must explain how loudness can grow faster than normally, it must explain overrecruitment and the on-effect, and it must not rely upon neural lesion. The fiber-loss theory, and the constant loudness-loss theory, cannot explain overrecruitment. The geometric theory cannot well be tested. The impedance theory is contradicted upon other grounds. The microphone theory is only an analogy. The duplicity theory can explain overrecruitment in the same terms of relaxing of inhibition as stated by Lorente de Nò, but it cannot explain the case of quick recruitment in a severely deafened ear, where all the more sensitive outer hair cells may presumably be changed. One or more of these theories may apply in a particular case. There is of course no necessity for a single theory to cover *all* cases of recruitment, since, as we have seen, a wide variety of conditions can give rise to it. The occlusion theory of Fowler and Lorente de Nò, however, still seems adequate to meet all demands. It is only necessary to emphasize that in this theory no neural damage is necessary, the effect being the same if certain sense-organ cells are defective.

ANALOGIES IN THE NORMAL EAR

Békésy (1) used alternate binaural loudness balance with partial deafness simulated by residual fatigue. Later workers have corroborated his pseudo-recruitment (10, 45, 46, 72).

The same technique with noise-masking to simulate degrees of deafness shows quick recruitment (45, 112, 113, 115, 116). The rate of growth of loudness in low tones vs. high tones has been cited as an analogue of recruitment (45, 119). An analogy in the cochlear microphonic has been cited (76). The direct exploration of

recruitment in the animal by a behavioral technique has yet to be worked out. Several techniques used in the clinic are adaptable to the animal as well, however, and when this is accomplished one may hope to see the physiology of recruitment illuminated more clearly than at present.

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ON THE INTERACTION OF SIMULTANEOUS RESPONSES¹

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In this discussion a theory will be developed to account for the effects of induced muscular tension upon learned and unlearned responses. It will then be shown that the theory permits evaluation of the role of tension in skill learning, and provides a rationale for the use of the blink rate as an index of generalized muscular tension. Finally, the theory will be related to the results of two recent studies of eyeblink conditioning to stress the immediate systematic importance of response interaction.

As the terms shall be used here, a muscle is tense if antagonistic effectors prevent it from shortening, contracted if the mechanical component is present as well. Muscular tension is said to be generalized if widespread muscle groups are active, focalized if just a few are involved. Many different methods have been devised for measuring muscular tension (26, 60). Some yield only indices of magnitude, but others show locus and patterning

as well. We shall emphasize the results of those investigations which have been designed to provide the more precise information.

By definition, both tension and contraction are properties of muscle that depend upon innervation. Distinctions between the two are largely arbitrary, for muscular activity is basically homogeneous. Thus a pattern of muscular tension, like a pattern of muscular contraction, is a response. Responses interact if they are modified upon simultaneous elicitation. Such modifications pose the general problem with which we shall deal.

TWO MOLAR LAWS OF INTERACTION

To begin with, there are two major factors which determine degree of interaction in artificial induction situations. First, interaction varies directly with the magnitude of the inducing response. For example, the amplitude of the knee jerk varies considerably as a function of changes in the activity of the other parts of the body (43, 59, 63, 111). Courts (17) has quantified this phenomenon. When his subjects squeezed a hand dynamometer with increasing amounts of force, knee jerk amplitude systematically increased.

Experiments by Freeman suggest another law, that interaction varies directly with the proximity of the simultaneous responses. He measured rate of finger oscillation and varied the locus of induced muscular tension. Maximal facilitation of rate was obtained if the adjacent finger was the site of induction (33, 35).

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In a further study finger oscillation was produced by stimulating motor points and again muscular tension was most effective if located in nearby muscle groups (36).

These facts can be related to information derived from the study of isolated responses. The degree to which a distant muscle group is involved in a response varies directly with the magnitude of the response and the proximity of the group to the maximally activated effectors. For example, Freeman (33) had subjects flex their fingers and recorded activity in other body parts. Changes were detected when the fingers were maximally flexed, but not when they were minimally flexed.

Hines (56) has pointed out that this increased distribution of muscular activity can be demonstrated by palpation. If fingers are flexed minimally, only muscles of the hand and lower arm are tense. If flexion is powerful, muscles as distant as those which extend the neck and adduct the scapula are involved. Shaw (87) has shown how extensive these changes can be by measuring the distribution of activity in gripping a hand dynamometer. He found changes in action potentials, many of them marked, in the opposite arm, hand, and leg, the foot on the same side, the mouth, jaw, nose, ear, chest, abdomen, and neck.

The proximity relation can be illustrated by results obtained by Davis (25), who measured electrical changes in the forearms and left leg of subjects while they worked mental multiplication problems or learned nonsense syllables. During multiplication subjects tend to move their writing hand, and Davis found that the changes were greatest here and least in the left leg. Smaller changes were recorded during nonsense syllable

learning, presumably because the focus of activity has been moved to muscles remote from all loci of measurement.

These facts show that interacting responses are overlapping responses. The factors which determine the probability that each of two simultaneous responses will involve a given muscle group are the same as those which determine degree of interaction. Our problem, then, is to account for the way in which overlap is translated into interaction.

The most parsimonious assumption is that interaction depends upon the convergence of simultaneous patterns of neural impulses. This hypothesis is the cornerstone of the present theory. Fortunately, the locus of the convergence is not difficult to specify, for the molar laws of interaction suggest that the event takes place in the nuclei of the motor system.

THE PLAN OF REPRESENTATION WITHIN THE MOTOR SYSTEM

Relevant to this conclusion are neurophysiological experiments in which the input to the motor system is systematically varied and the output of the system is observed. Usually the input is an electrical shock delivered to a cortical motor area. The output is analyzed through study of the muscular activity which results.

If the magnitude of the input to the motor system is kept at threshold value, the characteristics of the output vary as a function of the locus of the input. Points can be found in the cerebral cortex from which isolated movements of a finger, elbow, or toe can be obtained. A recent analysis of the precentral motor cortex of the monkey by Woolsey and Settlage (119) shows that there is a remarka-

ble correspondence between place in the motor system and place in the musculature. In fact, a chart of the motor cortex which summarizes the responses elicited from different points resembles a somewhat distorted, but intact monkey.

Similar information has been obtained for human subjects during routine stimulation of the cerebral cortex in neurosurgery. The most recent chart of threshold response data has been prepared by Penfield and Rasmussen (76). It shows a plan of motor representation which can be summarized by a somewhat disjointed figure with a small trunk and small legs, but large hands and feet. Centers for vocalization, salivation, and mastication overlap the ones for lips, jaw, tongue, and swallowing.

A further feature of the organization of the motor system is revealed if the stimulus is fixed in locus and varied in magnitude. Suppose that a point has been identified which, when a threshold stimulus is applied, gives an isolated flexion of the finger. If stronger shocks are delivered to this point, progressively more remote muscle groups are brought into play. The movement first involves the fingers alone, then the fingers and wrist, and then the fingers, wrist, and elbow. Very strong shocks elicit widespread movements. If a different point is stimulated, a similar sequence results in which the activity of a given muscle group depends with its proximity to the threshold group.

These data show that there is a spatial gradient of representation for each body part. A given movement is most readily obtained if a particular point in the motor cortex is stimulated, but around this focus there is a fringe region from which the movement can be elicited in combination with others. The closer

the fringe point is to the focus, the greater its sensitivity. Thus the motor channel for a body part varies in extent with the strength of the stimulus that is used in searching for the bounds.

It must be a rare input that fires only those cells which make up a representational focus. Distributed excitation within the motor system is the rule rather than the exception. The factors which are responsible for distribution deserve special consideration. Because of them, motor pathways are seldom discrete even when the input is sharply defined.

Input-output analysis yields none of the details of organization which are responsible for the overlapping somatotopic plan. Chang, Ruch, and Ward (14) suggest that one mechanism is cortical, that each muscle of the body is represented by a field of efferent neurons which varies in density from the focus to the fringe. When a focus is activated with a threshold stimulus, none of the fringe cells for neighboring muscles are fired. When the stimulus applied to the focus is more intense, excitation spreads and progressively more cells in the surrounding fields go into action.

Fibers within the cortex itself seem to be involved. In a study of the facilitation of a response to stimulation of one point by stimulation of a different point, Graham Brown (48) found that interaction disappeared if knife cuts were made between the two electrode positions. Foerster (32) reported that irradiation to the fingers could be prevented if a stimulated thumb area were completely surrounded by a cut. Finally, Erickson (30) found that epileptic seizures can be altered in course if a trench divides upper and lower halves of the sensorimotor cortex.

Distribution of excitation also takes place at lower levels. One line of evidence comes from examination of conditions under which a response to stimulation of one cortical point will recur when a different point is subsequently activated (72). Since cutting intrahemispheric connections has no effect upon such facilitation, it must be due to persistence in sub-cortical structures.

The fact that reflex irradiation (89) can be demonstrated in the bulbo-spinal preparation also indicates sub-cortical distribution. There is new evidence, too, which suggests that corticospinal tracts participate, for Glees and Cole (45) report that restricted lesions in the hand area of the monkey motor cortex result in degeneration of the tracts all the way down the spinal cord.

Feedback excitation from proprioceptive end organs within muscles is both a contributor and distributor of excitation within the motor system. Gellhorn (42) recorded muscle action potentials during stimulation of the motor cortex and found that magnitude of the electromyogram was greatly increased if the observed muscle was fixed in a lengthened position. Gellhorn (43) also showed that proprioceptive feedback from one muscle can increase the reactivity of neighboring muscles.

This feedback excitation undoubtedly affects several levels, and cerebral cortical areas are definitely involved. Woolsey, Chang, and Bard (116) stimulated the afferent spinal nerve roots of the monkey and found that the electrical activity is evoked in the precentral as well as the post-central areas of the brain. This somatic afferent projection is larger than the one defined by light tactile stimulation (118), for under the latter conditions the potential changes are

limited to the postcentral regions.

The new potentials related to deep sensitivity can be distinguished from the potentials evoked in the post-central region by a light tactile input. In the precentral gyrus they are so distributed that Woolsey, Chang, and Bard suggest that proprioceptive projection fibers terminate in the motor area for the same body part. But the effects of proprioceptive feedback are not restricted to the focus for the muscle which contains the receptor. It is a characteristic property of afferent systems that localized peripheral stimulation gives rise to activity within a considerable area of the cortex, with maximal changes in a central focal region (84, 105, 118, 121).

Thus the stage is set for proprioceptive feedback to alter the properties of two strategically important components of the motor system. Thus far the existence of motor areas other than the one in the precentral gyrus has not been considered, but actually there are at least four which supply somatic musculature (40, 41, 47, 76, 117, 120). The functions of the different systems have not yet been specified satisfactorily, but this is not essential for the present discussion. Although the characteristics of movements elicited from each area vary considerably, the overlapping somatotopic plan of organization has been found for all that have been studied in detail.

No attempt is made to evaluate the relative importances of these complementary distributive mechanisms. It should also be pointed out that the processes discussed are not the only ones which contribute to lack of definition within a motor pathway or to variations in width of a channel. The input to the motor system almost certainly is not dis-

crete unless it is artificial, and it probably varies in extent with changes in the magnitude of the output.

LIMITATIONS UPON THE INTEGRATIVE PROPERTIES OF THE MOTOR SYSTEM

The forms of movement which can be observed if the motor system is activated by an artificial stimulus have not yet been discussed, but the generalization is reasonably straightforward. If the input to the motor system is simple, the output of the motor system is simple. If a punctate, threshold electrical shock is delivered to an area for the wrist, we can observe flexion, extension, or deviation. The complexity of the movement can be altered by a change in the intensity of the stimulus, but the resulting activity of wrist, fingers, and arm has a limited resemblance to the movement involved in the throwing of a baseball. In short, a skilled movement is never observed, which suggests that the motor system has properties which cannot be altered by practice.

It has been proposed that the fibers which form a meshwork about the efferent cells in the sensorimotor cortex might be modified during acquisition, but Sperry (97) has shown that intersecting knife cuts in this region alter motor capacities of monkeys very little. Evidently the fibers contribute little to the patterning of a response. There is a noteworthy parallel between Sperry's findings and the surprisingly small changes in the spatio-temporal organization of human responses after therapeutic section of the corpus callosum (10, 94, 95, 96, 113). Perhaps a distributive role can be assigned to both classes of fibers. There are facts of bilateral interaction which add further support to the notion.

The Sperry data, unless one postulates an unacceptable independence of integration and neural connections (62), indicate that the pattern of the output of the motor system is largely dependent upon the pattern of the input. This hypothesis is hardly new; it is similar in conception to Tower's (107) keyboard analogy for the pyramidal tract. It is supported by clinical data and by experiments like those of Loucks (64) and Harlow and Bromer (52).

In summary, it is assumed that the motor system is not involved in the modifications of the central nervous system which take place during the acquisition of a skill. The motor system is a transducer which makes definite contributions to the synthesis of movements, but the properties of the system are fixed. Cerebellar loops are included within the system as defined here, and from the periodicity of motor behavior (53, 77, 100, 115), it is concluded that they operate as an input-informed feedback mechanism (83).

INDUCTIVE FACILITATION OF UNLEARNED RESPONSES

Consider next the interpretation of the Courts (17) experiment in terms of the principles just discussed. When the patellar tendon is tapped a motor channel of definite extent is activated, and thresholds of neurons in the vicinity of this channel are altered. But the input does not capture extensor neurons alone because of the overlap within the motor system and by the lack of sharp definition within the input itself. Hence the occurrence of extension is a statistical proposition; the input activates a population of cells in which there is greater representation of extensors than of flexors.

When the dynamometer is

squeezed another channel is activated, and its extent is proportional to the force of the grip. Excitation is distributed to the vicinity of the leg channel, and trips off some of the neurons that are near, but not at threshold when hand activity is absent. It has no effect upon the cells that are triggered by the input for the knee jerk.

The differential recruitment of extensor neurons is a function of the changes brought about by the patellar input, for the contribution of distributed excitation from the hand is relatively minor and cannot be expected to have a selective effect. Now, it is apparent from the spatial gradient of representation that the fringe regions adjacent to the leg channel are predominantly extensor in function. Similarly, the closer the cells are to the pathway, the more they are changed by the patellar input. Hence distributed excitation from the hand channel fires more extensor than flexor neurons, and the result is an increase in jerk amplitude. Facilitation increases up to the point where as many flexor as extensor neurons are recruited. An inversion of the function takes place if and when distributed excitation fires the relatively remote pools of neurons that are dominated by flexor cells.

With an assumption that reaction criteria are met when a critical population is brought to threshold, the same concepts account for the fact that induced tension alters the latency of a response (35, 38), or the rate and magnitude of a repetitive response. For example, finger tremor rate increases under load, and French (39) reports that finger tremor amplitude goes up when subjects squeeze dynamometers with the opposite hand. Body sway is more pronounced if one stands with the muscles of the

legs or body generally tensed (92, 93). No attempt is made to account for the periodicity of these responses; the treatment is limited to alterations of responses that are already established.

In summary, induced tension facilitates the responses just discussed by making the motor system available to an input, and not by operating upon the input. The theory is an obvious extension of concepts introduced by Sherrington (90) in his classical treatment of spatial summation. It can account for changes in the magnitude or the latency of a response. Presumably there are no other kinds of inductive facilitation.

INDUCTIVE FACILITATION OF LEARNED RESPONSES

If all that muscular tension can do is alter the magnitude or latency of a response, it is obvious that one must forego the notion that it can have a direct effect upon the formation of habits. Several experiments which support the opposite view require reinterpretation. The pioneer study is that of Bills (2), who found that nonsense syllable performance improved if subjects squeezed dynamometers. Although Bills' trends were not marked, subsequent studies have amply confirmed them.

But Stauffacher (98) and Courts (18) have shown that the interaction is not always facilitatory. They varied the degree of induced tension and found that there is first an increase and then a decrease in facilitation as grip pressure increases. Under conditions of extreme induction, Courts obtained interference.

According to Freeman (34), the results mean that proprioceptive feedback from induced muscular tension lowers the thresholds of excitability for all levels of functional

activity in the nervous system. The increased excitation makes the whole brain work better up to a point where the system is overloaded. A similar viewpoint is sponsored by Bills (3).

Actually, however, there is little evidence for such widespread effects. There can be no doubt that proprioceptive input alters the level and distribution of excitation within the motor system, but here the fiber bundles are concentrated into a projection system. So far as correlational mechanisms are concerned, proprioceptive feedback is only one kind of input among many. Thus it is probable that any unusual properties of the proprioceptive input are a function of the peculiar position of the proprioceptive projection.

In a discussion of several alternative interpretations of the Bills experiment, Robinson (81) held that the results might be attributed to an increased readiness to react in all muscle groups. This theory is similar in many respects to one just proposed to account for the facilitation of simple responses. Courts (20) has noted that research has substantiated the spread of muscular tension to effectors other than those directly involved in the process of induction, but adds that no one has yet been able to show how such generalized tension operates to produce both facilitation and interference.

However, by holding that interference is produced by facilitation of irrelevant responses, it is possible to account for the phenomena observed in complex performance situations with the same theory that handles facilitation of the knee jerk. To borrow the terminology of Hull (58), the difference observed by Bills is one of reaction potential and not of habit strength.

If this hypothesis is correct, then

interactive facilitation cannot be demonstrated unless there is some differentiation of the input in favor of the criterion response. Once selection has taken place, a channel is activated with a fringe that has predictable characteristics. From the spatial gradient of representation, the neurons whose thresholds have been altered most would contribute to the response, for they are situated adjacent to the focus of activity. Hence small amounts of added excitation associated with hand activity are most likely to fire these neurons, and facilitation is the result. If more excitation is added, progressively more remote pools of neurons are recruited. Eventually performance declines because a given amount of induction recruits more inappropriate than appropriate neurons.

Obviously, the effects of induced tension cannot be the same throughout practice. If an input captures an optimal population, added excitation triggers only neurons which contribute to the production of errors. This conclusion is documented by the data of Courts (21). He studied changes in the functional relation between degree of tension and performance as learning takes place. The results for nonsense syllable learning and pursuit rotor learning were similar. The greatest amounts of facilitation were obtained early. The optimal degree of tension became progressively less and the detrimental effects of extreme tension became more pronounced as practice proceeded. Freeman (37) reports a similar finding in mirror drawing.

Evidently, then, muscular tension can be a valuable asset during the early stages of practice on a verbal or motor task, but a hindrance to skilled performance. Thus Russell (85) found that muscular tension

interferes with ball-tossing performance, particularly in men. For the skilled individual any instruction that enhances tenseness produces errors. In motor skills, error production usually appears as an increase in response variability.

Several studies illustrate this relation. Bousfield (9) found that tremor rate, amplitude, and variability all increase during ergographic work. Renshaw and Postle (79) report that pursuit tracking is impaired if the stylus is gripped tightly. Furthermore, Feldman (31) found that two measures of variability in a tracking task tended to increase as a function of the degree of induced tension.

It is difficult to reconcile these facts with results which Bills (2) reports for the task of adding columns of digits. He found that hand tension improves both speed and accuracy. The facilitation of latency is understandable, but only if accuracy remains the same or deteriorates. There is little consensus on this point, for Freeman (35) reports greater speed with less accuracy in continuous addition and Zartman and Cason (122) report no effect.

The data of Block (7) are of relevance here. She used hand pressure, foot pressure, and the two combined for the induction conditions; arithmetic problems, syllogisms, and analogies were the materials. Facilitation could not be demonstrated. This supports the assumption that tension cannot facilitate selective processes. Bills and Stauffacher (4) provide further evidence. Their subjects supported weights and solved arithmetic reasoning problems or analyzed short detective stories; the only differences were in latency of response.

In summary, consider an observation which, if duplicated with proper controls, is catastrophic for the present argument. Bills (2) presented lists of paired associates for a single trial under conditions of normal and induced tension, and then studied recall with no induced tension. The group which learned under tension recalled the list slightly, but significantly better than the normal control group. However, the list required only thirty seconds for presentation and the recall trial followed almost immediately. It is doubtful that relaxation was achieved and that induced changes were completely dissipated (29).

This experiment illustrates another necessary control if the facilitated response is learned. A response can operate upon the input for another only if its occurrence alters the stimulus situation to which the simultaneous response is conditioned. In the Bills experiment this factor should favor the group which both learned and recalled without tension, so the phenomena cannot be explained in this way. Under other circumstances a change in magnitude or latency might be attributed either to a change in the stimulus situation or to a loss of interactive facilitation.

It should be possible to design adequate experiments to isolate these factors. One approach to the problem can be derived from the assumption that strength of conditioning, as distinguished from response strength (55), should not vary as a function of changes in proprioceptive feedback. The latter argument follows from the results of experiments by Grant and Schneider (49, 50) on the effects of changes in the magnitude of conditioned stimuli.

SIGNIFICANCE OF CHANGES IN THE DISTRIBUTION OF MUSCULAR TENSION DURING LEARNING

The foregoing theoretical analysis can be extended to learning situations in which artificial tension is absent. Under these circumstances the criterion response can be visualized as interacting with all the others, even though the latter may be difficult to define separately. However, these extraneous responses are unlike an induced response in that they are eliminated as a function of practice.

The sequence of elimination is such that tension changes are from generalization to focalization (28). Sometimes this process can be traced not only to a single limb, but within the limb itself as well. For example, Renshaw and Schwarzbek (80) found that in hand pursuit learning the trunk muscles first relax, and then those of the shoulder, upper arm, and lower arm in order. Distant muscle groups are frequently involved in the changes. Freeman (33) noted a progressive reduction in the tension of a leg muscle as subjects learned a pursuit task or nonsense syllables.

Changes in focal muscle groups are complicated. The input for the response increases with practice, but distributed excitation derived from irrelevant responses decreases. These facts preclude simple interpretation of studies in which measures of tension are derived from the active limb. In this group are the investigations of Daniel (22), Telford and Swenson (103), Stroud (99), and Ghiselli (44).

Recall that facilitation by induction is most pronounced early in learning, that the optimal amount of tension is progressively less as learning proceeds, and that late-in-learning tension interferes with performance. The ideal conditions, then, are such that tension is maximal early in

learning and declines progressively as a function of practice. Note that these conditions prevail when there is no artificial tension, even though the amount of tension at a given point in the practice series might be below the optimal level. Early in learning the musculature is widely activated, and the responses eliminated first interact the least with the criterion response. Elimination results in loss of facilitation, but at the same time the occurrence of the criterion response is less dependent upon it.

Muscular tension is typically generalized under most of the conditions which we commonly associate with difficult performances. This relation was first pointed out by Duffy (28), and its use in the analysis of work situations has been explored by Ryan, Cottrell, and Bitterman (86). Factors thus far identified are lack of practice, prolonged work periods (1, 82), distractions (23, 73), complex materials (24, 88), and increased incentives (15, 35). According to the present theory, in each of the instances cited additional tension contributes to response elicitation.

It is a common practice for flying teachers and other motor skill instructors to stress the value of relaxation to their students. Yet it is evident that a positive, as well as a negative, role can be assigned to the tenseness of individuals who have just begun to learn. To say that tenseness should not be tampered with may seem foolish to an individual who has long associated the effortless with the expert performance, but the available evidence indicates that optimal conditions for the novice are not the same as those for the skilled performer. A datum directly related to this conclusion has been reported by Davis (25), who found that instructions to relax the right

arm interfered with nonsense syllable learning.

THE BLINK RATE AS AN INDEX OF GENERALIZED MUSCULAR TENSION

In most of the experiments just cited, elaborate instrumentation was employed to detect changes in the distribution of muscular tension as learning proceeds. However, several lines of evidence support the conclusion that the blink rate can be used as an index of degree of generalization. Telford and Storlie (102), for example, had subjects learn a mirror-drawing task. They found that there is a reduction in the blink rate as a function of practice.

This observation can be correlated with the fact (76) that the locus of the motor channel for the eyelid is in a particularly strategic position. It is bordered on one side by the massive structures for the tongue and face; on the other by the huge representation for the hand. Hence a man makes few responses "without batting an eye." In animals, Blount (8) notes that blink rate increases during certain infrequent facial movements, e.g., yawning, sneezing, and eating. Thus, the blink response is facilitated by simultaneous activity in neighboring motor channels, and most important motor channels fall in this category.

Several other phenomena support this interpretation. Difficulty is a factor which alters distribution of tension, and Clites (16) found that blink rate varies with the difficulty of a nonvisual task. Recitation of the alphabet brings about an increase in rate, but the effect is more pronounced if the recitation is backward. Other data show a positive relation between the blink rate and another distributive factor, the incentive for performance (15).

In the latter experiment, blink rate measurements were taken in the interval between trials in a tracking task. The particular conditions are emphasized because the frequency of blinks during performance was almost zero. The task required continuous visual control, a condition which inhibits the blink rate. Drew (27), for example, studied the blink frequency of subjects who tracked target or drove an automobile. He observed that the blink rate decreases as a subject moves from heavy traffic onto the open road, or tracks a target which follows a course of increased complexity.

Luckiesh and his collaborators (68) have studied the effects of visual working conditions upon the blink rate. Their task, reading, did not require continuous scrutiny. Illumination, type size, presence or absence of glare, and the visual correction worn were variables related to the rate of blinking. Significantly, Luckiesh and Moss (66, 67) have also found that changes in muscular tension accompany changes in glare and illumination. The relationship of blink rate to illumination during reading is in question, however, for MacFarland, Holway, and Hurvich (69), Tinker (106), and MacPherson (71) have failed to confirm the trends.

Bitterman (5) studied the effects of type size and with Soloway (6), the effects of auditory distractions on the blink rate of subjects given a variant of the Minnesota Test for Clerical Workers. They were unable to demonstrate any significant changes. Bitterman and Soloway pointed out that auditory distractions have been shown to be accompanied by increases in muscular tension, and concluded that muscular tension does not affect blinking.

But, as Luckiesh (65) points out,

direct comparisons between his results and those of Bitterman cannot be readily made. In Bitterman's visual task, pairs of identically or dissimilarly spelled words are presented to be judged alike or different. This material lacks the redundancy of English prose, for each letter must be examined if performance is to be efficient. Even in reading there is a reduction in the blink rate (104), and this change takes place in spite of the fact that blinks tend to occur at periods and page turns (51).

Other kinds of visual work are not so conveniently parcelled. MacPherson (71) found that the blink rate, as compared to a control condition, went down most for counting rows of dots or for detecting aircraft silhouettes, somewhat less during reading, and least during the plotting of graphs. MacPherson concluded that the blink rate is inversely correlated with the ease of the task. But, since Clites (16) found the opposite relationship in a nonvisual task situation, the effects of difficulty are probably obscured by the requirement of continuous visual input.

Since fatigue is one of the conditions in which muscular tension is distributed, studies of blink rate in relation to prolonged visual work are relevant. As one might expect in a situation where antagonistic processes operate, the results are variable. Hoffman (57), Carpenter (13), and Tinker (106) report increasing blink rates; Carmichael and Dearborn (12), Simonson and Brozek (91), and Brozek, Simonson, and Keys (11) find no impressive trends. Wood and Bitterman (114) point out a fact which accounts for most of the differences: the blink rate does not change if the subject knows that this performance is being measured.

Only the Carpenter data cannot be

handled by this hypothesis. They are understandable, however, if related to observations made with the Tufts alertness indicator (110). This instrument has been designed to provide automatic signals for human beings who might go to sleep under dangerous circumstances. It measures muscular action potentials from just above the eye. The choice of site was an empirical one, for it was found that the activity of this muscle reflected the level of activity in other effectors. Why this should be so is evident from the present theory.

Travis and Kennedy (108) showed that as action potentials from the eye decline, hand reaction times to visual and auditory stimuli go up. Similarly, Kennedy and Travis (61) demonstrated that action potentials and tracking performance are correlated. The same investigators (109) found that the index varies with the task, and significantly for the problem at hand, that it is high during simulated lookout performance that requires a hand reaction.

In the Carpenter study, the subjects were tested with the Mackworth apparatus (70), which requires a key-pressing response each time a pointer on a dial moves a double step instead of a single step. Thus Carpenter's situation is one in which more excitation is distributed to the eyelid mechanism than under reading conditions and the marked effects observed are to be expected.

Many factors must be considered by anyone who attempts to use the blink rate as an index of generalized muscular tension. Nevertheless, the index has many advantages. There are savings in cost and time required for securing and analyzing records, and no equipment need be attached to the subject. Furthermore, the subject need not be aware that meas-

urements are being taken, and so the blink rate index is particularly adaptable for use in clinical experimental situations.

FACILITATION OF THE ISOLATED BLINK

The present theory predicts that induced muscular tension should have an effect upon both the blink rate and the blink reflex. There are no data of this kind so far as rate is concerned, and the status of the reflex problem is far from clear. Freeman (37) reported that the eyelid reflex is facilitated by tension induced in the lower limbs, and Peak (74) obtained facilitation of blinks to loud sounds by induced hand activity. Courts (19), however, found no facilitation by hand tension if blinks were elicited by a puff of air. Peak (75) suggested that Courts' responses were maximal to begin with, that his subjects were not fully relaxed, or that time intervals are important. Tye (112) performed experiments to control these factors, and used both sounds and puffs of air for eliciting the blink. He interpreted his data in support of Courts.

However, it is probable that such facilitation was a factor in a recent experiment by Taylor (101). She administered to a population of college students an inventory of "manifest anxiety," and then studied eyeblink conditioning in two groups of subjects chosen from the tails of the distribution. She found that the frequency of conditioned blinks was greater in the anxious than in the nonanxious group.

One criterion of anxiety is an unusual amount of muscular activity. The supposition is evident in the choice of items for the inventory used by Taylor. This, together with the

fact that the eyeblink representation is strategically located, suggests that the Taylor effect is a change in reaction potential attributable to response interaction. Taylor reports in a footnote a fact that is absolutely, if not statistically, gratifying: of the four subjects discarded because their unconditioned blinks were too frequent, three were classified as anxious.

The supposition that has been advanced is strongly supported by data obtained by Ponder and Kennedy (78). They studied the blink rates of subjects who were excited or angry, and found that the interval between blinks was sharply reduced. But another observation of these investigators is even more relevant. They went to a court of law and counted the blinks of individuals who were giving testimony. These people blinked much more frequently during cross-examination—a situation which would make almost every one of us anxious.

One final argument can be offered in support of the notion that Taylor's data represent an instance of interactive facilitation. It follows from the present theory that conditioned discrimination should be more difficult to develop in anxious subjects, for the facilitation should prolong the elicitation of a response that is being extinguished. This deduction has just been confirmed by Hilgard, Jones, and Kaplan (54). They used Taylor's inventory to obtain anxiety scores for a group of subjects. Subsequently an eyelid response was conditioned to light, and then a conditioned discrimination was developed between two different positions of the conditioned stimulus. There was a significant positive correlation between anxiety scores and lack of discrimination.

SUMMARY

In review, the theory that has just been developed attributes the interaction of simultaneous responses to a convergence of impulse patterns upon the motor pathways of the central nervous system. It is nothing more than a combination of classical concepts of spatial summation and modern information on details of organization within the motor system.

The principal conclusion to be drawn from this analysis is that interactive facilitation can alter the magnitude or latency of a response, but has no direct effect upon response selection. One corollary of this notion is that induced muscular tension affects rate of acquisition by promoting response elicitation, and not by altering correlative mechanisms.

Interference with performance is held to result from a facilitation of competing responses. A basis for differential recruitment of motor neurons is found in the spatial gradient of motor representation for each body part. Changes in the effects of induced muscular tension from facilitation to interference are assumed to be related to changes in the probability that increments in excitation will facilitate more appropriate than inappropriate motor neurons.

It has been pointed out that the change in distribution of muscular

tension during learning is invariably one of focalization. From this it is concluded that the degree of natural tenseness varies in same way during learning as does the optimal degree of induced muscular tension. Since a positive, as well as negative, role can be assigned to this tenseness, instructors in motor skills should abandon the practice of stressing the value of relaxation.

A rationale has been developed for the use of the blink rate as an index of generalized muscular tension. Variations in the rate are attributed to excitation distributed to the motor channel for the eyelid from neighboring channels for the head and hand. Finally, it is suggested that similar facilitation accounts for the results of two recent studies of eyelid conditioning in anxious subjects.

For psychologists, the present argument is a molecular theory. We do not assume that molecular concepts are better than any other kind, but they seem to yield the most parsimonious interpretation of the data under discussion. We think that it has been shown that molecular theory can predict molar behavior and, furthermore, that molecular theory can indicate the minimum features of any molar theory. For example, in the Hull system there are many constructs, but one seems to be missing and required. If the present theory is correct, it could best be labelled *efferent neural interaction*.

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BOOK REVIEWS

BECK, S. J. *Rorschach's test*. Vol. III. *Advances in interpretation*. New York: Grune & Stratton, 1952. Pp. viii+301. \$5.50.

Dr. Beck has high standing among those who study the Rorschach in America, because of his steady interest in improving the technique and his desire to communicate clearly his procedures and theories. His new book presents his method of interpretation more completely than before. Those who are using the test or training others in it will find great use for the clear and intelligent discussions of theory and of four case analyses. All four of these persons were under treatment, but represent patients from the community rather than hospital patients. For two cases Beck has repeated tests, and for all cases has clinical notes made during treatment.

The "advances" in Beck's statements do not represent a marked change in viewpoint. As always, he insists upon regarding the examination as an opportunity to sample behavior and thought production, not as a device for generating scores. He makes this clear by commenting that Tredgold, with the Mare and Foal Test, could make the same type of dynamic interpretation. This point of view might seem to cut the ground from under most of the talk about interpretation of Rorschach per se; as would also the recent report that in interpreting the adjustment shown in protocols, a qualified psychologist who knew nothing about Rorschach could agree with trained Rorschachers as well as they could agree with themselves.¹ Beck, however, con-

siders the Rorschach performance an unusually good source of revealing cues, and this book is an attempt to make those cues explicit for other testers.

The cues now employed differ little from those discussed in his earlier books. He elaborates more clearly than before and distinguishes, for instance, between the significance of a cue in patients and the interpretation of the same cue in superior nonpatients. Beck has explored many new leads, such as the Levy-Zubin analysis of movement. He points out nuances in each type of response which should qualify the interpretation of the main scores. He does not reverse any earlier interpretations.

This in itself should suggest that the book is disappointing. We have now had thousands of research studies, some well conducted, which have failed to establish validity of many interpretations commonly made. One would expect such evidence to be used in revising the interpretative scheme, or that positive evidence would be advanced to demonstrate the validity of Beck's statements as to the equivalence of test behavior and personality structure. Beck does suggest that this might be a later step, but many readers would prefer to find a psychological proposition accompanied by, or preceded by, the reason for affirming it.

Beck discusses in a few pages current research on the test. He criticizes some studies for trying scoring innovations instead of validating Rorschach's Rorschach test. These in-

¹ GRANT, Q., IVES, V., & RANZONI, J. H. Reliability and validity of judges' ratings of

adjustment on the Rorschach. *Psychol. Monogr.*, 1952, 66, No. 2 (Whole No. 334).

novations have, I think, rarely been sufficient to obscure the significance of positive or negative findings.

His second criticism is more substantial, that attempts to validate single signs or scores do not take into account the interactions between cues that an interpreter might use. It is apparent, however, that Beck in this book makes many statements about the significance of single scores, i.e., about main effects. While these interpretations would be qualified by added facts from other scores or qualitative features, such additions are embellishments to a main interpretation that he implies would be true more often than false in persons generally.

We find Beck turning his back on research evidence in the way that is too common in the literature and in the conversation of clinicians, by saying, in effect, "Evidence or no evidence, these propositions have clinical validity" (p. 43). In view of Beck's desire to make his *operations* public so that the Rorschach test will be a part of psychology rather than a cult, it is regrettable that he does not make his *validation* public. He does not see that so-called clinical validity is a type of private, or subjective, validity and hence not acceptable save as a source of questions to be tested by research.

The matter of color shock will serve to document this criticism. The literature now contains several studies which make it doubtful that color shock is truly color shock, or that it has the correlates claimed for it. Beck however treats color shock in the traditional manner, going further to regard color shock as evidence of anxiety regarding present temptation, while shading expresses guilt regarding past misdeeds. A rather attractive rationalization for this

position is offered. Beck then dismisses the negative findings of objective research on color shock by citing Siipola and others as if their work negated the other studies and supported his position.² An examination of the original will disclose that their findings do not substantiate conventional procedures for measuring color shock.

Before the Rorschach can hold an established position among those who are deficient in the "wish to believe," the interpretative statements such as Beck presents must be accompanied by explicit evidence. Such evidence would not say (to quote at random), "This inability to react to color is the mark of the person insensitive to the world's exhilarating values" (p. 45). It would say, "Persons who show no responses scored *C*, *CF*, or *FC* are judged to be emotionally unresponsive by such-and-such criteria in *x* cases out of one hundred." Here is the crucial problem. Beck and others word statements as if generally true, although the context makes it clear that the interpretations are not true in 100 cases out of 100. Hence the propositions are not to be accepted as stated. This approach to the Rorschach does not leave us with testable claims. The Rorschach enthusiast can say that the truth is contained only in his many qualifications. But this too easily becomes an evasion of responsibility. If one tries to take all the qualifications into account, he arrives at a unique pattern of scores and cues for each record. One cannot find a reasonable number of cases with this pattern, and therefore cannot make the required assessment of the statement's *degree* of correspond-

² SIIPOLA, E., KUHN, F., & TAYLOR, V. Measurement of the individual's reactions to color in ink blots. *J. Pers.*, 1950, 19, 153-171.

ence to reality. To verify a proposition we must know its probability of being true. As the propositions become more complex, they become unverifiable. If the main effects are indeed assumed in interpretation, it is *their* degree of validity which must be reported.

So much stress on what Beck's book lacks reflects my sense of proportion, not Beck's. He gives almost no space to these issues, compared to the extensive discussion of cases. Those discussions can be evaluated, perhaps, by some other clinician who would report whether they agree with his private experience. There are few persons who can match Beck in experience, or in the care with which he traces his own conscious thoughts. A reader might choose to test the statements in terms of plausibility. On this ground, Beck stands high. He gives a long train of argument to support each interpretation, rather than issuing it as from an oracle. He is painstakingly self-critical in specifics, while not being so critical of his major premises.

Judged from the viewpoint of interest and the quality of Beck's thinking about personality, the book is admirable. The book is frequently poetic. It is written with far more style than most of its contemporaries, and many passages are quotable. In criticizing Lewin's disinclination to the genetic approach, for instance: "Lewin's deficit for this kind of construct was that he was not clinically trained. He can think in two dimensions. But his anxiety as experimentalist blocks his moving into the third" (p. 11). Throughout, Beck does a good job of expressing in hundreds of illustrations the concept of personality as an interaction of many forces and perceptions within the person. Whether the Rorschach

is a dependable tool or not, a book like this affords an excellent basis for thinking about the extreme complexity of personality. It is a powerful antidote for an oversimple view of personality as described by a few common traits or factors. The book will perform this service, even for many Rorschachers, by debunking naive interpretation of the psychogram or the single numerical score.

In summary, the book appears to be a fine one for Rorschach users or students who plan to continue with the test. Beck has rich experience to report, and the fact that it is subjective does not prevent its being useful to other interpreters. The book will not change the opinion of those who presently regard most claims for the Rorschach as rationalizations or statements of faith. The book will be of little assistance to the minority who regard the validity of the test as an open question which must be settled by determining which interpretations hold up in what proportion of cases. Beck has an extensive research program in process, and it may be that his reports from them will belatedly give us reason to have confidence in the present book.

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SHOSTROM, EVERETT L., & BRAMMER, LAWRENCE M. *The dynamics of the counseling process*. New York: McGraw-Hill, 1952. Pp. xvi+213. \$3.50.

This book is largely a how-to-do-it manual of counseling procedures in the setting of a student personnel program. Labeled *self-adjustive counseling*, the approach is a compounding of nondirective and directive viewpoints flavored with a dash of learning theory. The goal of

counseling is increasing self-direction on the part of the client. Highlighted is a structured sequence of client-centered interviews preceded by a group-orientation session and culminating in a synthesis interview. Records, tests, and vocational information are used when desired by the student.

Although the approach is claimed to encompass educational, vocational, and personal problems of adjustment, the reader will find that major attention is given to what might best be described as nondirective educational-vocational guidance. Neither psychotherapy nor projective tests is mentioned. "If the counselor feels he is counseling a student with problems too involved for his competencies, turning the student and his records over to the psychologist would be the safest action" (p. 106).

Two chapters are devoted to discussion of a control-group study designed to test the effectiveness of the self-adjustive approach vs. the traditional approach. Ratings of terminal evaluative interviews with regard to client satisfaction and feelings of self-direction favor the self-adjustive approach. Details such as number of cases are not provided.

While this readable little book contains many helpful suggestions, references, and devices, it is obviously too nontechnical and practical to serve as a basic text in the graduate training of clinical psychologists. For a broader audience, the book should be very useful and have wide appeal.

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KERR, MADELINE. *Personality and conflict in Jamaica*. Liverpool, Univer. of Liverpool Press, 1952. Pp. xiii + 221. 15 s.

This is another contribution to the growing literature in the field of culture and personality. Kerr has written a most interesting account of the effect of divergent culture upon the formation of personality. On the one hand, the natives of Jamaica are expected to conform to the standards and ways of life set down by the English who have dominated the island for more than a century. On the other hand, the Jamaican natives have their own culture which is often at odds with that imposed by the British.

Kerr defines personality "as a constellation of roles which the individual learns to play under given conditions" (p. 196). The basic difficulty with the native arises because the roles which he is expected to play are defined variously from the two cultures to which he is exposed. Among the many difficulties which the Jamaican faces in this world are first of all the prejudices related to color and class.

There is essentially a three-level class system in the island. At the bottom are the great mass of peasants and farm and city workers, most of whom are definitely Negroid. At the second level is the middle class made up of business groups, some professionals, and individuals who have made a success of farming. At the top is the social elite composed of higher officials, plantation owners, and other people of material substance and traditionally high family standing. The chapter entitled "Class and Color" points out how the attitudes toward color play a part at all class levels but particularly with people who have some Negro blood but who are trying to move upward in the social strata. Apparently the individual tends to react to color in terms of any given social constellation. "One day a person will

be bitter and anti-white, yet when another constellation is involved he will be concerned with fears and dislikes only of people darker than himself. It is as if in some constellations he is the almost white man with white ideals, in others he is the dark man resenting white domination" (p. 96).

The divergent cultures and what they do to the individual are nicely brought out in the discussion of the educational program. The native children are exposed to a curriculum borrowed from Great Britain. As a result the content of the courses is "mainly concerned with issues and facts which cannot possibly have any relation" to the pupils' everyday life. For example, they learn the botany of plants that they have never seen, nor ever will see, and the history of parts of the world that have absolutely no meaning to them. On the other hand, their own local history and materials on their own habitat are completely neglected.

With reference to the development of the personality, Kerr provides a wealth of interesting and informative material. She points out that the spontaneous extroverted characteristic of early childhood gradually gives way to a rigid and introverted pattern found in adolescence and in adulthood. The wells of spontaneous creativity tend to be dried up, the recreational life of the children, particularly that in the school, is practically nil, and the adolescent and the adult tend to develop an apathetic and withdrawn type of behavior.

The close relations of mother and child are examined and the important role of the grandmother is described. The latter is familiar with the magical practices associated with childbirth, supervises the event, and carries out the magical rituals which are deep in their culture. Kerr believes that the

place of the grandmother is a more or less natural consequence of the mother-oriented family in Jamaica. The function of the father is often rather tenuous, especially in the peasant class. A high proportion of these couples are not legally married and the husband and father often plays a quite incidental role in child care and discipline.

As might be expected, there is a considerable fantasy life among these people but, with few exceptions, it is not directed into creative activity. Rather, it runs off into supersitition, magic, and a great concern for religious emotionalism. There are outbreaks into trancelike states; there is much interest in religious revivalism, and occasionally there is an outright outbreak of violence. Another interesting feature of the fantasy life of the native is the rich verbiage to which he is addicted. Apparently this is an aspect of Negro culture everywhere, although no one has ever adequately explained it.

Another possible factor in this situation which is mentioned but not explored by the author concerns the high incidence of malaria and hookworm among these people. We know that apathetic behavior and a low level of energy expenditure follow from the recurrence and persistence of these two diseases. It seems to the reviewer that we might have here an interesting interplay of constitutional factors and a conflicting culture. That is, if these people were living in a more or less unified culture, the effect of malaria and hookworm might be evident, but their effects on the personality might be different. This hypothesis could be tested by some cross-cultural comparisons.

Finally, a word may be said about the methodology of this study. The data were principally from two sources. First, Kerr interviewed a

large number of people of all classes and kept protocols of such interviews. In addition she gave a number of projective tests to children and to a few adults. The projective techniques used were the Rorschach, the Lowenfeld Mosaic test, drawings, and a projective test devised especially by the author for Negro children. In general, the results of the tests confirmed the findings from the interviews.

The author closes her comments on the relationship between field work and the test results in words which serve as a summary statement of this book. "In general the tensions caused by economic frustration, color prejudice, wrong methods in education, produce social conditions of insecurity, both economic and psychological, of doubt over role functions and of encouragement of magical beliefs. These conditions in turn produce a basic personality of an unintegrated type" (p. 193).

KIMBALL YOUNG

Northwestern University

PIAGET, JEAN. *Play, dreams and imitation in childhood*. (Trans. by C. Gattegno and F. M. Hodgson.) New York: Norton, 1951. Pp. ix+296. \$5.00.

The current work by Piaget is another stimulating and provocative contribution to the literature on the development of children's thinking. In this well-translated volume, Piaget has as his basic goal an explanation of the evolution of "representative activity," which is "characterized by the fact that it goes beyond the present, extending the field of adaptation both in space and in time" (p. 273). Such an activity is essential in reflective thought as well as in operational thought.

Two theses are presented by Piaget

in the book: (a) the transition from rudimentary, primitive, and situational assimilation of experience to the operational and reflective adaptation of experience can be studied by the analysis of imitative behavior and play activity of the child from very early months of the life; and (b) various forms of mental activity—imitation, symbolic activity, and cognitive representation—are interacting.

Underlying his analysis is the assumption that two fundamental processes operate in development: assimilation and accommodation. Assimilation refers to the idea that "no new external element ever gives rise to perceptive, motor or intelligent adaptation without being related to earlier activities" (footnote, p. 80). Accommodation refers to the adaptation to the demands of reality. By virtue of both these processes the individual evolves schemas, which are considered by Piaget as elementary structures in the psychological life of the individual. New experiences of the organism are brought into these schemas by the assimilation and the accommodation processes. Assimilation and accommodation gradually achieve equilibrium in the course of growth, since equilibrium is achieved at the completion of growth.

With such an over-all conception as a guide for analysis, which is dynamic in nature, Piaget proceeds to show how imitation, play, and representative activity evolve. Consistent with his previous writings, we find here an evolutionary presentation with rough age groupings given for the various stages of development. Incidentally, the same general ranges are given as in previous writing, i.e., seven seems again a crucial year for the beginnings of certain well-defined stages of growth and eleven to twelve the beginnings of adult-like activity.

In the first section of the book he deals with imitative behavior. He presents evidence from the observation of his own children as data. The protocols are presented in the text at varying length as illustrative of his various "facts" and theoretical points. He shows that imitation follows through six stages until "true" imitation occurs which is a consciousness of imitation as well as an imitation controlled by the intelligence as a whole.

Following this is a discussion of play, in which the same type of data is presented. A theoretical consideration is given, using the assimilation and accommodation principles as basic guides. Stages of play behavior are described, which proceed from a purely functional pleasure stage which is primarily assimilative in nature to the eventual development of organized and socialized games with definite rules. Details of this evolution are discussed. An analysis of symbolic play is presented, which Piaget considers as primarily egocentric in nature. He also discusses the nature of the symbolic behavior and claims this derives clearly from the child's own thought, an assimilative activity.

In a treatment of secondary symbolism as seen in dreams, play, and unconscious activity, Piaget presents a penetrating analysis of classical Freudian thinking, taking issue with such concepts as the censor mechanism, the mechanism of repression, and infantile memory. His argument, however, is constructive in that he integrates some of the Freudian thought of ego, repressions, and the unconscious with his own explanations. Jung's concept of the relative generality of symbols and of symbolism as primitive language and thought is also included in this explanation. Finally, Piaget comes to a

treatment of representative activity—cognitive representations—and shows the steps in development from the sensory-motor schemas to conceptual schemas. A step-by-step evolution is attempted.

The observations notwithstanding, this book seems to be largely speculative. It is packed full of provocative ideas and challenges, and certainly Piaget has fearlessly explored an area of great import. Some general limitations are apparent. There is little reference to current American or British research relevant to the area of play and symbolism. For example, there is no reference to modern theorizing and research in ego psychology and the relevant contributions from play therapy. Second, the speculative nature of the book limits its translation into testable hypotheses for replication or in some instances even additional data gathering by other psychologists. Third, the evidence presented by Piaget leaves much to be desired in terms of rigor, objectivity, and at times detail. Finally, the style is heavy, repetitious and even esoteric, which taxes the reader's comprehension and interest. Nevertheless this book offers a contribution to a vital area, the development of thought, which is in desperate need of investigating and has been too long neglected by American psychologists. Piaget has certainly trodden with a heavy step where many of us have feared to venture.

IRVING SIGEL

Merrill-Palmer School, Detroit

CATTELL, RAYMOND B. *Factor analysis. An introduction and manual for the psychologist and social scientist.* New York: Harper, 1952. Pp. xiii+462. \$6.00.

In his preface Cattell states that this book has been written to meet

the needs of (1) the general student who wishes to learn what factor analysis is about, (2) the instructor who wants a textbook in factor analysis as part of a course in statistics, and (3) the research worker in this field. There are, to be sure, several excellent books on the subject of factor analysis but the author feels that they are too advanced mathematically or too specialized for general use.

The book is divided into three parts. Part I deals with the general theory of factor analysis and its application as a scientific method. Factorization is represented as valuable in a preliminary clearing of the ground and as useful also in providing a better understanding of the variables which emerge from an experiment. Ways in which factors may be obtained from a matrix of correlations and the purpose and necessity for rotation are presented with emphasis upon the meaning of factor loadings. The thesis is developed that factors may be thought of as source traits when simple structure has been established. In addition to the usual procedures in which factors are obtained from the intercorrelations of tests (called *R* technique) or from the correlation of persons (called *Q* technique), Cattell introduces two new methods, the *O* and *P* techniques. In the *P* technique factors are determined from the correlations of test scores achieved by the same person over a period of time. When the time periods instead of the tests are correlated we have the *O* technique. In this terminology of matrix algebra *O* is the transpose of *P*. Both of these methods should be useful in studying trends over a period of time.

Considerable attention is given to the need for oblique factors in Part

II, and methods are provided for the extraction of orthogonal and oblique factors by the centroid method. Cattell does not overlook other factor methods, but he places the main emphasis upon the centroid as being the most flexible and generally useful. In this judgment the reviewer concurs. The final section of the book is concerned with various problems which arise in the factorization of measurements of ability as well as of personality.

Cattell has met his third objective, namely, that of preparing a handbook for research workers, better than his first two. The difficulty level of the book is not adapted to the non-mathematically trained student. It is doubtful whether the beginner can read profitably beyond the first two chapters, and most graduate students in psychology will understand very little of Part II unless it is preceded by a review of analytical geometry and matrix algebra. The problem of rotation as presented in Chapter 12 is unnecessarily discursive; and the student is not likely to get from Chapter 13 a clear idea of the real need for and value of oblique factors. While Cattell has clarified the distinction between reference axes and factors, his treatment is hampered by a confusing terminology. Rules laid down for carrying out a technique are not sufficiently well illustrated to make the rules readily applicable. As an example, the procedures to be followed in making a multiple group centroid analysis (pp. 178-184) are set down without illustration in 11 steps and 8 supplementary notes.

This book is a real contribution to the literature of factor analysis and should be read by everyone seriously interested in research in this field. The final chapter on short-cut pro-

cedures will be especially valuable to the research worker. A feature of the book is its sprightly style, occasionally refreshed with a touch of humor.

HENRY E. GARRETT

Columbia University

WALLS, GORDON L., & MATHEWS, RAVENNA W. *New means of studying color blindness and normal foveal color vision. University of California Publications in Psychology*. Vol. 7, No. 1. Berkeley: Univer. of California Press, 1952. Pp. iv+172. \$2.50.

In Walls and Mathews' lively, unorthodox, and iconoclastic monograph on color blindness, Figure 1 shows a schematic set of chromatic response curves for the three-component theory of color vision. It is offered, according to the legend, "only as an aid to the reader who is not an expert in the field." Let the nonexpert, however, beware. The introduction includes detailed discussions of several theoretical issues basic to the experiments and interpretations developed later in the work. There is a modification of the Young-Helmholtz three-component theory, one which the authors have labelled the "excess hypothesis"; an interpretation of protanopia as a reduction or "loss" system and deuteranopia as a fusion or "collapse" system, an eclectic idea presented some years ago by Pitt; a discussion of the genetics of the more common types of color blindness; a history and critical evaluation of the entoptic phenomenon known as "Maxwell's spot," which leads the authors to reject the orthodox view that it results from absorption by macular pigment; and finally the novel idea is presented that the entoptic Maxwell pattern is due ex-

clusively to the nonuniform distribution of redness, greenness, and blueness receptors in the fovea.

The experimental portion of the monograph describes a battery of four "tests" applied in the analysis of color vision and its defects: the filter anomaloscope, the so-called "3-light test" (a simplified means for making gross luminosity measurements), neutral point determinations, and the use of a purple dichromic filter for eliciting the Maxwell spot phenomenon. The latter, which constitutes an entoptic demonstration of the receptor-type distribution pattern (RDP), is reported to be characteristically different in protanoids as compared with normals, and completely absent in deuteranoids. The third and final portion of the book summarizes the results of the differential diagnoses provided by these tests and offers detailed case analyses which are heavily interlarded with genetic interpretation and speculation.

At a time when conformity dominates the social and intellectual climate it is refreshing to find long accepted views and assumptions vigorously challenged. Furthermore, the authors' strictures about the indiscriminate and improper use of macular pigmentation as an explanatory concept are very much in order, and their claim that the RDP and neutral point determinations can serve as the basis for making a clear-cut diagnosis between protanopes and deuteranopes should stimulate further inquiry along these lines.

Unfortunately, the text sometimes suffers from selection and misinterpretation to support the authors' bias. In reviewing this work, moreover, a number of questions arise, all basic to an evaluation of the various hypotheses and findings presented.

For example, why should we believe that "When a protanope (or a deuteranope either, for that matter) calls something blue, one can 'take his word' for it. He is experiencing blue" (p. 87) when we have been told on page 6 that "to take the protanope's word for it . . . that the long-wave hue he calls yellow is the normal's yellow experience, is like asking a congenitally deaf man whether Brahms's Fourth or Beethoven's Third is the greater symphony." If we accept the conclusion that "Nothing is found to conflict with the supposition that the basis of each of the hereditary types of color blindness is retinal and receptor" (p. 156), are we to discount the subject who is diagnosed as "a victim of *central protanopia*" (p. 130, their italics)? If we accept as valid the conclusion that "Besides standard (loss) protanopia, and central protanopia (in an occasional heterozygote), *collapse protanopia* apparently exists" (p. 162, their italics), aren't we stretching beyond useful recognition the basic explanatory dichotomy which says protanopia = loss, deuteranopia = collapse? If we are told (pp. 31-32), with respect to the size of the rod-free area, that "The most recent and best values today are those of Rochon-Duvigneaud (1943)" (30' to 40'), are we to ignore the fact that Rochon-Duvigneaud's 1943 text refers only to measurements originally reported by him in 1907, and that Polyak's authoritative modern research, which is not even mentioned by Walls and Mathews, gives a conflicting value of 1°40'? When we read "No conceivable amount of macula pigment could possibly change the hue of a monochromatic light for anyone, be he tartanopic, normal, or whatever" (p. 42), are we to deny the

existence of the Bezold-Brücke hue shift with change in intensity? If Walls and Mathews seek to reconcile Ahlenstiel's conflicting data with their own by gratuitously assuming that nearly all of Ahlenstiel's 94 color defectives were protanoid (p. 89) and that his normal group must have included many deuteranomals (p. 36), should we grant these assumptions on pure faith? If, for an observer like Ba. L., who "can make out no RDP at all," we are offered the conclusion (p. 131) that "she does have an objective, retinal, receptor-type distribution pattern like her twin's, or more likely like their mothers' . . ." but that "she has no basis for perceiving it entoptically," what real diagnostic value does the entoptic receptor distribution pattern retain?

This jungle of fact and fancy is indeed stimulating but it is clearly for the specialist.

LEO M. HURVICH

Eastman Kodak Company

BERRIEN, F. K. *Practical psychology*. (Rev. Ed.) New York: Macmillan, 1952. Pp. xv+640. \$5.00.

What is practical psychology? Impractical psychology? What does practical itself mean? Dr. Berrien answers the first question by implication in what he includes in the text. Answers to the latter two questions remain unanswered. In *Practical Psychology* the author covers selectively what others have called applied psychology, and what, still earlier, was subsumed under the label of psychotechnology. Dr. Berrien has organized his book around five major areas or topics: problems of adjustment, industrial psychology, applications to consumers and advertising, crime, and personal problems. The first and last

listed topics, however, might well have comprised a single part of the book. Certainly the two chapters covering personal problems, one on vocational guidance and the other on effective speaking and writing, would not be out of place if added to such chapters as study efficiency, mental health and guidance, and adjustments in later life—these latter being among those comprising the first part.

Turning to specifics, the good features of the text are many; the bad ones relatively few. Dr. Berrien has done a splendid job of collating and summarizing empirical findings on work efficiency and accidents, on psychological research in advertising, and on the general area of psychology applied to crime. All chapters abound with relevant illustrative material. The undergraduate student, the intended consumer, will derive great benefit from a careful reading and translation into practice of what he finds on "how to study" and "principles of mental health."

On the negative side, the most pervasive shortcoming is the failure to set a theoretical matrix for the discussion of each topic. For example, in the chapter on training in industry and business, virtually all the discussion deals with the organization and mechanics of training without much attention to the fundamental learning process for which the organization and mechanics are ancillary. Similarly, in treating morale, no conceptual framework (e.g., communality between individual and group values, group-shared goals, or Maier's frustration principle) is given within which to interpret the specific findings cited, say, with regard to what workers want to get out of their jobs. In short, the book is replete with "what" and "how," but relative-

ly sparse on "why." Patently, content and methodology in our rapidly expanding field are more subject to obsolescence in the light of new findings than the underlying why of behavior.

The book represents a major revision of the pre-World War II edition. The changes comprise rewriting and expansion, rather than thorough coverage of the more recent literature. While current citations occur, they are far outnumbered by early references. The author has turned out a readable, teachable book, which should be useful at the undergraduate level if curriculum organization includes a survey-type course in applied psychology. It will be in competition with the similar works of Burt, Hepner, Husband, and others.

W. J. E. CRISSY

Queens College

VINACKE, W. EDGAR. *The psychology of thinking*. New York: McGraw-Hill, 1952. Pp. vii + 392. \$5.50.

Although psychologists have long been concerned with the psychology of thinking, we find in the literature little systematic presentation and evaluation of the over-all problems of thinking. Thus the event of a text in this area should be of great interest. Unfortunately, we have here little more than a survey of literature. The author at the outset points out that he "has not been guided by preference for any particular species of theory" (p. vii). This becomes only too apparent when in the body of the book we find him using constructs from opposing points of view and unsuccessfully trying to reconcile them. This makes of course for a most eclectic and, at the same time, confusing presentation, too typical of many of our text books.

A clear-cut definition of thinking

is never offered by the author. In the introductory chapter he offers Warren's definition with some analysis and criticism, but does not clearly commit himself regarding its acceptability. Throughout the book, there are various statements which might be considered definitions of thinking, and which presumably represent the author's view. First, "Thinking is the use or reorganization or application of what has been learned" (p. 42); next, it is "... manipulation of the environment wholly or partially without overt activity... there is always some symbolic activity" (p. 58); and also, "Thinking serves in the adjustment of the organism to its environment, both internal and external" (p. 6). The book's organization and the author's justification for it do allow us to gather something of Vinacke's concept of the psychology of thinking. He distinguishes first of all between reasoning and imagination as two kinds of thinking, the former being more related to the external world and more controlled, the latter less so. Second, all thinking is personalized and shaped by selective and regulatory systems which are established through learning. These systems include concepts, attitudes, sets, traits, and motives. Finally he accepts the field theory interpretation of mental activity "... as occurring in the midst of a complex pattern of nervous activity" (p. 5). The larger divisions of the book contain a section on Reasoning, another on Imagination, and a third on Personalizing Functions of Thought. The subdivisions include chapters on concept formation, logical thinking, transfer, problem-solving, autistic thinking, creative activity, and internalization of experience.

In the chapter on the mechanism of thinking the author subscribes to

the motor theory of thinking, offering Watson's view as one acceptable to him with only slight modification. He says, "Thinking consists in the reactivation of past experience by means of the implicit activities of the muscles originally involved in that experience" (p. 69). As evidence for this theory he offers the works of Jacobson and Max and others which have reported that action potentials can be obtained in the parts of the body about which the subject is instructed to think. He does recognize the possible criticism that such evidence does not prove that the movements are essential to the thought but may only be accompanying phenomena. In rejecting this interpretation, he offers the evidence which Jacobson reported, on the basis of one case, that a man with an amputated limb could not imagine or think of activities in that limb. There is since the work of Jacobson, however, considerable work on phantom limb phenomena which argues against the author's interpretation.

The eclecticism and resulting confusion are most profound in the chapters on problem-solving, transfer, and the internalization process. In discussing transfer and retroactive inhibition he offers Katona's research as a valuable contribution in the area of transfer, particularly because of Katona's emphasis on the role of understanding and meaning. Next he offers a number of behavioristic notions about transfer and retroactive inhibition, and then with apparent enthusiasm devotes a section to the gestalt concept of trace systems and the forgetting process. He then concludes that "There is, however, no real conflict between the more static interference theory, and the dynamic, field view. Rather they complement each other" (p. 150). A

footnote on the same page states that there is essential agreement between the psychoanalytic and field theories in the interpretation of the forgetting process.

Again, Vinacke states in the section on internalization of experience that conditioning forms the basis for all the early training of the infant. At the same time, he points out that it is difficult to explain "long-term" learning by conditioning, as it is rather specific and subject to extinction. Conditioning then sets the groundwork upon which other learning mechanisms, such as "symbolic functions," take over. How they take over or how they are learned is never too clear. Furthermore, a discussion earlier in the book of the need for recentering and re-organizing in problem-solving situations is never reconciled with the concept of conditioning. At the end of one chapter he does leave the reader with a provocative question in this connection: "To what extent is recentering in problem-solving linked with, or independent of, generalization gradients?" (p. 155). In general then, it can be said that if Vinacke has a point of view which actually does bring into harmony the concepts he uses, he has not stated it explicitly.

Despite all of the above criticisms, this book has much of positive value to offer. The objections to an inadequately developed and organized point of view should not take away from its potential value in the classroom. It is a very well written book, and does contain a well-organized and rather complete survey of the literature. Some chapters are handled especially well, such as "Creative Activity" and "Ideas, Imagery and Imageless Thought." The latter part of the book devoted to imagination and the personalized aspects of

thought appears to be the best.

IRENE R. PIERCE

Wellesley College

AUSUBEL, DAVID P. *Ego development and the personality disorders*. New York: Grune & Stratton, 1952. Pp. xi+564. \$10.00.

Ausubel's first purpose, presumably, is to construct a comprehensive nonpsychoanalytic theory of personality and of the personality disorders. To this reviewer, his theory is "comprehensive" as a listing of the current and standard criticisms of Freud, and "nonpsychoanalytic" in rejecting id and superego. That is, his original contribution to theory is a questionable modification of what Freud called "mental topography."

However, he does cover a wide selection of the criticisms directed at psychoanalytic theory. He reformulates, redefines, rephrases, and reannotates in the light of recent knowledge. His suggestions are often sound and may make the psychoanalytic model more meaningful to psychologists and more amenable to objective research. Also, his analysis of "identification"—a major feature of the book—is thought-provoking, especially in his specific developments.

In regard to topography (id-ego-superego), Freud said: "It must not be supposed that these very general ideas are presuppositions upon which the work of psychoanalysis depends." Apparently Freud saw his rough mapping of the "mental apparatus" only as a crude model or analogy, convenient for communicating certain ideas. Ausubel offers an alternate model, largely in terms of ego development. In other respects, his theory is at least "Freudomorphic." For example, he objects to Freud's use of the term "instinct" and to his

pansexual emphasis. However, his revisions in such details, while extensive and familiar, fail to effect any essential change in the psychoanalytic model.

In contrast to Freud, Ausubel would put less emphasis on id-ego conflict and, perhaps more important, less emphasis on conscious-unconscious conflict. In contrast to Mowrer, he would put less emphasis on ego-superego conflict. He reduces personality dynamics to intra-ego conflict. He would have us see conflict and psychodynamics largely in terms of difficulties arising in various stages of ego development. Tendencies which are appropriate to one stage of ego development must be abandoned or modified for wholesome development in later stages. If the abandonment or modification is not satisfactorily accomplished, the residual tendencies—no longer appropriate—lead to both internal and external difficulties in adjustment. The degree of consciousness or unconsciousness is less important than Freud supposed. In general, instead of id-ego-superego disagreements, he sees less precisely labelled stages of ego development (never reified!) bargaining, conflicting, and compromising.

In dealing with Freud's topography, Ausubel's approach may be illustrated by his treatment of the id. Instead of the *id*, he uses such terms as (1) visceral needs, (2) regal omnipotence, and (3) hedonistic urges. The strength of visceral needs is related to body type; Sheldon's viscerotonic endomorph may have stronger needs than the ectomorph. Presumably much of what the analysts see as *id* may be explained in terms of the impatient "regal omnipotence" of very early infancy, which develops more or less universally because the parent does every-

thing for the newborn child. However, this unreasonable expectation of quick gratifications is best understood as an early aspect of ego development. Hedonistic urges cover in general the early and lasting tendency to short term goals and immediate gratifications. This tendency is partly axiomatic and partly a learned pattern of early stage ego development. The analysts' *id* concept contaminates this tendency with other, misleading implications (instinct, sexuality).

Ausubel's focus is the ego. He might have advanced theory further if he had abandoned ego as well as *id* and superego. To him the ego is not the person, it is an identifiable abstraction "referring to the inter-related set of experiences, perceptions, attitudes, motives and values which revolve about the awareness of self." If he had dealt with the individual functioning unit, the person, as his focus (in a present environment and specific culture, with a given heredity and personal history, etc.), he might have gone further in pulling together advances in personality theory. However, while he does see the individual at times, he is usually dealing with ego as ego, an artificial abstraction representing only one aspect of the person as a unit.

The ambitious attempt, the relative success with which he channels varied contributions to his purpose, and the occasional originality in development of details make this book of interest to the sophisticated reader with time to read it. More important, Ausubel may be expected to make further significant contributions to personality theory in the future.

ROY M. HAMLIN

*Western Psychiatric Institute and
Clinic
University of Pittsburgh*

HOOKE, DAVENPORT. *The prenatal origin of behavior*. Kansas: Univer. of Kansas Press, 1952. Pp. viii+143. \$2.50.

This small volume is based on the Porter Lectures delivered at the University of Kansas. Its author is one of the world's most distinguished and original students of the development of behavior. He summarizes the early growth of activity in infra-human vertebrates and the sequence of fetal activity in human embryos and fetuses. He gives special consideration to the neural basis of the responses which he describes.

The author's own experimental work leads him to the conclusion that the behavior of vertebrates, including man, has its beginning in extroceptively initiated responses. External stimulation, it is further shown, releases a succession of responses in various vertebrate organisms which are basically similar. In general, he favors the view that behavior is individuated out of a more general matrix rather than being the integration of previously more simple reactions. All relevant experimental work is carefully reviewed in connection with this problem. Especial attention is given to the points of view of Coghill and Windle.

Hooker is notable among those who have studied the early development of behavior because of his careful description of the stimuli which he has used. In his discussion of Carmichael's work reference to the stimuli used is also given in some detail. The fact that Carmichael used the method of von Frey in calibrating hairs in units of tension in which both pressure and radius are considered is not mentioned, however. This may seem to be a small point, but an agreement upon the best form of tactile stimuli to be used

in behavior work is not without importance.

The role of the endocrine organs in fetal life is considered. An interesting diagram recently developed by Humphrey is given of the probable reflex pathway for contralateral neck and upper-trunk reflexes in the human embryo.

The author has performed a service for all students of behavior, and especially for psychologists, in bringing together in a clear and concise manner much of the scientific material which bears on the origin and development of behavior. The reviewer feels that the present volume should be in every psychologist's library.

LEONARD CARMICHAEL

Tufts College

MIKESELL, W. H., & HANSON, G. *Psychology of adjustment*. New York: Van Nostrand, 1952. Pp. ix+406. \$4.50.

This is a text designed for those courses which are growing in popularity and titled variously, but often called Personal Adjustment or Mental Hygiene. The expansion of these courses can probably be attributed largely to students' demands for assistance in understanding themselves and directing their lives, and to the growth in colleges of group approaches to personal guidance. In evaluating the recent texts in this area several factors must be considered: introductory psychology texts are expanding the content devoted to personal adjustment; there is a growing literature on individual and group therapy and on student-centered teaching, which has a very direct bearing on this psychological self-help project; moreover, psychologists as scientists have turned a properly critical eye on the handling of this relatively new service; last,

it seems that we are in the midst of a transition in our goals and methods of teaching this kind of material.

It appears then that the task of writing a personal adjustment text today is a difficult one. A new text in this area should be expected to make a contribution on the subject beyond that offered by the best introductory volumes. It should lend itself to newer methods of student-centered teaching by making good didactic treatment secondary to planned stimuli for self-discovery and personal development. This would also include the inculcation of more realism, some training of the student to recognize and resolve basic life conflicts, and a gradual substitution by him of effective outlets for his self-defeating habits and attitudes.

Where can the Mikesell and Hanson text be placed in respect to these difficult goals? The table of contents suggests a systematic organization. Chapters on basic concepts such as frustration, conflict, and "wants" launch the book. There follow seven chapters in common problem areas: sex, education, work, family. The remaining chapters, with topics such as therapy, choice of mechanisms, and confidence, have a therapeutic thread running through them. Most of the concepts introduced are defined in terms of several currently used text books.

This apparently systematic treatment breaks down as the book is examined more closely. Drive, urge, need, force, and motive are used in various places without indication of the authors' evaluation of them as synonymous or different aspects of the total motivational process. The authors do not distinguish between drive and instrumental act in emphasizing urge-mindedness rather than

goal-mindedness as a hygienic set. This emphasis on drives and needs rather than on final goals is good, but it is not applied often in the book. Renunciation and direct living, for example, are advised without much emphasis on how they are motivated and achieved. Defense mechanisms are not evaluated well enough in terms of the contexts in which they may be found. Hobbies, reading, writing, and occupation are evaluated as withdrawal mechanisms without fully emphasizing the conditions under which these same outlets may be creative. The relationship between the concepts used is not always clear. One of the main characteristics of maladjustment presented is "misfitness" which seems tautologous. Maladjustment is characterized in terms of symptoms instead of personality structure.

In discussing their objective the authors claim that they will introduce theory only to bring out practical points. They purport to present facts about and remedies for the errors of human living, yet usually several definitions of a basic concept are given instead of a simple one favored by the authors. Nine classifications of "wants" are presented. In fact, there is considerable unevenness throughout the book in the writers' attempts to be academic or practical. For example, parts of the chapter on therapy appear to have been written from a descriptive and academic standpoint rather than with the view of arousing therapeutic practices in the reader.

The book contains much that the authors have undoubtedly found helpful to students taking their courses, when it was presented in an effective teacher-student context. On the whole the text is quite traditional

in its approach. It does not differ greatly in approach from many existing texts in the field.

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BERNARD, HAROLD W. *Mental hygiene for classroom teachers*. New York: McGraw-Hill, 1952. Pp. xxi+472. \$4.75.

Bernard considers mental hygiene as a point of view which requires that education provide the most adequate satisfaction of and a minimum of conflict between the fundamental human needs of all individuals. He clearly demonstrates how all aspects of the school, the curriculum, teaching methods, "discipline," and grading have a bearing upon mental hygiene. Constructive and more healthy procedures are recommended to replace the many familiar and traditional ways of doing things which are inimical to good mental health. The teacher's mental health, as the principal factor influencing adjustment of pupils, is given extensive treatment.

Mental hygiene is also represented as a set of techniques. One of the major purposes of this text is to make these techniques available to every teacher, since there are many books for use of the specialist and for those "concerned with guidance as an approach" to mental hygiene. As Bernard claims, the services of the specialist are not available to many schools, particularly smaller ones. While respect for the psychologist and psychiatrist is shown, the student using this book might get the impression that such specialists are not really needed. The teacher is told not to expect to succeed with all children because adjustment is influenced by factors other than the

school, but the fact that a specialist might succeed where the classroom teacher fails is not mentioned in this connection. The reference to the "formal guidance approach" indicates an alignment with many educators who fail to see that a school which is organized to meet the needs of the child has the guidance approach whether it is formalized or not. It is safe to say that no school can fully meet the needs of all children without the help of specialists, whether they be psychologists, psychiatrists, or guidance counselors. One gets the impression that the specialist may perhaps deal with the "abnormal" child, but when the teacher asks, "How can I tell if a child is abnormal?" the answer seems to be that "it all depends."

Perhaps the most confusing statements are those having to do with marking. Presumably mental health can be fostered better by "evaluating" than by "marking." This may be true if one assumes, as Bernard seems to do, that there can be only poor marking and that the faults of the curriculum may be attributed to marks. Bernard would like to see children evaluated in terms of their capacity, but never considers what will happen when the dullard who "has done as well as could be expected" finds that he has not done well enough to be the physician or engineer he aspired to be. *One important need for mental health is the fortitude to face reality.*

Considerable space is devoted to the use of art, literature, play, and drama as techniques for understanding children and providing avenues of expression and personal fulfillment. "... the classroom teacher can use any or all of these approaches if he has only the willingness to experi-

ment." But more is needed than a yen for experimentation, which seems to mean "try it and see what happens." Of course, what Bernard quite properly means is that the teacher should be willing to break with tradition, but little psychological foundation is given in terms of which the teacher may judge the value of what happens when innovations are tried.

Very little reference is made to research literature but there is wide quotation from reputable authorities. Further readings are suggested at the end of each chapter, many of which

are hortatory. Some old references might well be replaced by newer ones. Study questions and visual aids are listed for each chapter.

The psychologist could well use this book in a supplementary fashion in courses for teachers because of its close touch with their real problems. Its use by an instructor without adequate psychological preparation and clinical experience, however, might well lead to the belief that fostering mental health is much simpler than it is.

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